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MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH OTHER

SELECTED AND ABSTRACTED PAPERS.

VOL. LXXXIII.

EDITED BY

JAMES FORREST, ASSOC. INST. C.E., SECRETARY.

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CORRIGENDA.

Vol. lxxxii., p. 339, line 27, *for* "6 inches" *read* "by 6 feet."
" " p. 341, " 3, *for* "12-feet" *read* "12-inches."
" " p. 386, " 11, *for* "1881" *read* "1871."
" " p. 387, " 4, *for* "Associate Member" *read* "Associate."

THE
INSTITUTION
OF
CIVIL ENGINEERS.

SESSION 1885-86.—PART I.

Sect. I.—MINUTES OF PROCEEDINGS.

19 May, 1885.

Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

(*Paper No. 2070.*)

"On the Theory of the Indicator and the Errors in Indicator-Diagrams."[1]

By Osborne Reynolds, M.A., LL.D., F.R.S., M. Inst. C.E.

Section I.—Introduction.

In 1856 Hirn published an experimental comparison of the indicated work, with the work done on the brake, and came to the conclusion that, whatever might be the cause, the indicated work was too small, being only just equal to the brake-work, leaving no margin for the air-pump and the friction of the engine.

This conclusion of Hirn's seems to have excited little notice. Rankine mentions it in "The Steam-engine," but expresses doubt whether it accords with subsequent experience, particularly that of marine engines.

Since that time many engine-experiments have been made. It does not appear, however, that these have been with a view to verify the indicator, but rather that the indicator-diagrams have been taken as data from which to determine the efficiency of the engines; nor has, so far as the Author is aware, any definite theory of the disturbances to which the diagram is subjected as yet been published.

The importance of studying the disturbances, or, in other words, the errors in the diagrams, becomes evident, when it is considered to what an extreme extent the indicator is now trusted to give a true measure of the work on the piston. In ninety-nine cases out of every hundred, there is absolutely no check within 20 or 30 per cent. In some classes of engines (winding and pumping) the work they are performing is of a measurable kind, but rarely or never is the work measurable to within 5 or 10 per cent. The only work which is definitely measurable is that done on the friction-brake as used by the Royal Agricultural Society; and even then, although the brake may give a measure of the actual work to within 1 per cent. or less, it does not furnish a check on the indicator to within from 5 to 20 per cent., for between the work measured by the indicator and that measured by the brake, is the unknown work done in overcoming the resistance of the engine. This, which varies from 5 to 20 per cent., is an absolutely unknown quantity, except in so far as it is found by subtracting the brake-power from the indicated-power, and hence furnishes no check within its own magnitude on these quantities.

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[1] An abstract of this Paper has been given in Vol. lxxxii. of the Minutes of Proceedings, p. 232. The discussion upon this Paper was taken together with that upon the following one.

There is thus absolutely no check on the indicator, which is now made the sole standard, not only of the performance, but of the value of engines. Considering what this means in mere money, where, as in the case of marine engines, large sums often depend on a margin of power which is a very small percentage of the whole, it becomes evident how important it is that the exact extent to which these instruments can be trusted should be well known. Yet in spite of Hirn's warning, the results of the indicator appear to be accepted without question, solely on the ground of their general consistency, of the simplicity of its apparent action, and the excellence of its construction.

On close examination, it appears in this case, as in others, that the apparent simplicity of action is due to the obscurity of certain facts; for example, the possible stretching of a piece of string; and that, taking all the circumstances which may affect the diagram into account, its action is by no means a simple matter. It may be that, in some cases, these disregarded circumstances only produce an inappreciable effect, but even this cannot be known as long as they are disregarded.

The theory of the indicator has now been taught for many years in the engineering classes in Owens College, Manchester, and the calculations to a certain extent have been verified by experiments on the College engine. This engine, though by no means of a high class, has been rendered well adapted for this purpose by the addition of a brake-dynamometer and a speed-indicator. It has long been the Author's intention to publish this theory, but this has been deferred for want of time to make a sufficiently extensive series of experiments. Last year Mr. Brightmore, Berkeley Fellow in Owens College, Manchester, undertook the experiments and carried them out very successfully. The results of his investigation appear to be of considerable importance, and as their interpretation depends on the theory, an account of this is submitted, to be read in conjunction with a Paper by Mr. Brightmore.

For the diagram to be exact, it is necessary—

1. That the pencil of the indicator shall, under every change of pressure, instantly move through a distance in exact proportion to the change of pressure in the cylinder of the engine.

2. That the paper on which the diagram is taken shall change its position in exact accordance with the change of position of the piston of the engine.

The first of these is accomplished, so far as it is accomplished, by holding the piston of the indicator by a spring carefully adjusted, so that the deflection is proportional to the load; and as there is no great difficulty in making a spring such that this proportion shall be maintained so long as the temperature is constant, and in making the instrument so that the temperature of the spring shall be 212° Fahrenheit, there is no reason to suppose that the indications of the indicator are not within 1 per cent. of the forces at each instant deflecting the spring.

But in order that these indications may correspond with the pressures of steam, it is necessary that there should be no other forces acting on the spring. Such forces, however, arise from the inertia of the weights to be moved and the friction, notably that entailed by the necessity of pressing the pencil on the paper.

In assuming the indicator as accurate, it is supposed that the forces resulting from inertia and friction are too small to be perceived; whether this is so or not, can only be ascertained by considering these forces.

The second of these conditions of exactness is accomplished by connecting a revolving drum, by means of mechanism, with the piston of the engine, so that, if there is no yielding in the mechanism, the drum will revolve through distances exactly proportional to the distance moved by the piston of the engine. There is no difficulty in arranging mechanism which will secure the corresponding motion of two bodies, if the forces can be kept constant on the mechanism. This is attempted in the indicator by pulling the drum in one direction by a spring, and connecting it with the piston by means of a cord wound round the drum, so that the spring always keeps the string in tension. Since all strings—in fact, all matter—is elastic, in order that the position of the drum may always correspond with the position of the engine-piston, it is necessary that the spring shall exert a constant force in all positions of the drum, and that there shall be no other forces.

As a matter of fact, however, the springs used do not exert a constant force, the force increasing as the drum is moved against the spring; and further, there are forces, namely, the forces arising from the inertia of the drum and the friction of the mechanism, principally of the drum on its supports. The diagram will, therefore, only be accurate in so far as these unequal forces are small; and the effect of these forces can only be ascertained after careful consideration.

It thus appears that there are five principal causes of disturbance; two of these (1) and (2) affect the motion of the pencil, and three (3) (4) and (5) the motion of the drum.

(1). The inertia of the piston of the indicator and its attached weights.

(2). The friction of the pencil on the paper and its attached mechanism.

(3). Varying action of the spring.

(4). Inertia of the drum.

(5). Friction of the drum.

These will be separately considered.

SECTION II.—DISTURBANCES ON THE PENCIL.

(1.) *The effect of the inertia of the Pencil and its attached Mechanism.*—This, although obvious enough in a general way, presents the same problem as the planetary disturbances, which can only be definitely expressed by means of some form of mathematics. As the general solution of the problem is well known to mathematicians, and is unintelligible to those who are not, it will be best here to omit all the steps, and to proceed at once to the results, about which there is no question.

These results may best be expressed in symbols, of which the meaning is as follows; taking lbs., feet, and seconds as general units, then put—

i for the indicated pressure at any instant;

p for the actual pressure corresponding to i;

w for the weight of any particular piece of mechanism attached to the pencil;

r for the ratio which the motion of this weight bears to the motion of the piston of the indicator;

W for $\Sigma (r^2 w)$ where Σ expresses the sum of all the quantities in the brackets;

g for 32·2, the acceleration of gravitation;
e for the number of lbs. to the inch on the diagram;
a for the area of the piston of the indicator in square inches;
s for the ratio of the motion of the pencil to that of the piston of the indicator;

In Richards' indicator—

$$a = 0 \cdot 5;$$
$$W = 0 \cdot 33;$$
$$s = 4.$$

For other indicators these may be found by measurement.

The relation between i and p, in as far as it is affected by inertia, is expressed by the equation—

$$\frac{W}{12\,e\,s\,g}\,\frac{d^2 i}{d t^2} + a\,i = a\,p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The general solution to this equation is well known, and without going into detail, it will be sufficient to give the solution for the case, which is, N being the number of revolutions of the engine per minute—

$$i - p = (p_1 - p_3)\,\frac{\pi^1 N^2 W}{900 \times 12\,a\,e\,s\,g}\left\{A_1 \sin \frac{\pi N}{30}\,t + A_2 \sin \frac{2\,\pi N}{30}\,t + \&c.\right\} + C \sin \sqrt{\frac{12\,a\,e\,s\,g\,t}{W}}. \quad . \quad . \quad (2)$$

t expresses time in seconds;
p_1 greatest pressure;
p_2 least back-pressure;
A_1 A_2 are coefficients depending on the shape of the true diagram;
C is a constant depending on the disturbed state of the pencil.

From equation (2) it appears that the effect of inertia is to cause two disturbances, corresponding to the two terms on the right hand side. These may be considered separately.

The first term has the factor

$$A_1 \sin \frac{2\,\pi N}{60}\,t + A_2 \sin \frac{4\,\pi N}{60}\,t + \&c.$$

which will go through a complete cycle when t changes by

$$\frac{60}{N},$$

that is, by the time of revolution of the engine in seconds. This disturbance will be the same during each revolution of the engine, and will be called the cyclic disturbance.

Given the shape of the true diagram, it would be possible to determine A_1 A_2 so as to find from equation (2) the value of $i - p$. But this would be a very complicated piece of work for such an irregular curve as the diagram, and as the object is not so much to find the magnitude as to find when this is small, it is sufficient to consider a circular or elliptic diagram; for such a diagram it is found that the mean difference of i and p written $\overline{i - p}$ is given by

$$\overline{i - p} = \pm \frac{\pi^2 N^2 W}{12 \times 900\, a g e s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

the positive sign to be taken for the forward stroke and the negative for the backward.

If this effect were large compared with the mean acting pressure $\overline{\frac{p - p_3}{2}}$, then in all probablity the area as well as the form of a true diagram would be seriously disturbed; but if this effect is small, say 1 per cent. in the case of the oval, it will be small for the true diagram. Hence the increase of area is less than 1 per cent. so long as

$$\frac{2\, \pi^2 N^2 W}{12 \times 900\, a e s g} < 0 \cdot 01,$$

and from this it is found that the cyclic disturbance may be 1 per cent. for Richards' indicator when N and e have the values in Table I., and as this disturbance increases as $\frac{N^2}{e}$, its possible values for all other cases may be found.

TABLE I.—ENGINE-SPEEDS at which the ENLARGEMENT of the DIAGRAM by INERTIA BECOMES 1 PER CENT. with the RICHARDS' INDICATOR USED in this INVESTIGATION.

Scale of Diagram in lbs. to an inch.	Number of Revolutions. Richards.
20	166
30	203
40	237
50	262
60	288
70	312
80	332
90	352
100	371

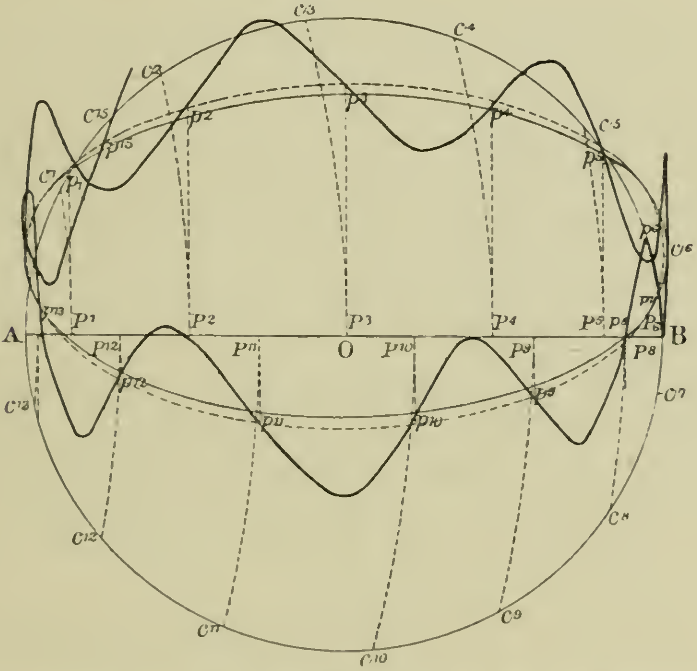
In the case of the oval or circular diagram the effect of this cyclic disturbance would be to increase the vertical diameter, as shown by the dotted line in Fig. 1. What it would be on the true diagram is very difficult to express, except to say that it would be to round-off all corners and increase its size much in the same way as in the oval.

The second term in equation (2) represents a disturbance which goes through its cycle in an interval of τ seconds where

$$\tau = 2\pi\sqrt{\frac{W}{12\,a\,e\,s\,g}} \qquad (4)$$

This may be called the vibratory disturbance. The period represented by τ is that in which the pencil vibrates when disturbed. Such disturbances are introduced by the departure of the diagram from the true ellipse.

FIG. 1.



The result of such disturbance is shown by the waving line in Fig. 1.

The time occupied in completing each one of these waves as from p_1 to p_3 is constant, viz. τ equation (4).

Hence the number of waves in a complete revolution is given by

$$n = \frac{\frac{60}{N}}{2\pi\sqrt{\frac{W}{12\,a\,e\,s\,g}}} \qquad (5)$$

For Richards' indicator—

$$n = 486\,\frac{\sqrt{e}}{N}.$$

In the diagram, owing to the unequal motion of the engine-piston, the lengths of these oscillations increase from the ends to the middle. If, however, a circle be drawn on the atmospheric line A B, having the extreme length of the diagram as diameter, this may be taken to represent the crank-circle on the same scale as A B represents the stroke. Then if the points p_1 p_2 &c., in which the waving line cuts the mean line, are first projected perpendicularly on to A B in P_1 P_2 &c., and then P_1 P_2 projected by means of a radius to represent the connecting-rod on to the crank-circle in the points c_1 c_2 &c., it will be found that the arcs c_1 c_2, c_2 c_3, are all equal, since the crank turns through equal arcs in equal times.

But for the effects of friction these oscillations, once set up, would go on for ever; so that even at low speeds a fair diagram would be impossible.

By friction the oscillations are gradually destroyed, so that they are more or less localized to the neighbourhood of the points at which they are produced, *i.e.*, the points where the curvature in the true diagram is sharp, particularly at the point of admission where the rise of pressure being instantaneous acts the part of a live-load, and forces the pencil twice as far as it ought to go. This sets up a series of oscillations.

It is seldom that the time of oscillation is exactly commensurable with that of revolution, so that if all the oscillations set up in one revolution are not destroyed by friction before the revolution is complete, the pencil will not describe the same path in two successive revolutions, a fact frequently observed in diagrams taken from locomotives at high speeds.

The error which these oscillations cause in the area of the diagram depends on their magnitude, but also, and to a greater extent, on the smallness of n, the number in a revolution. But the evil of these oscillations is not so much an effect on the area, which, even did they exist to the extent shown in Fig. 1, in which n is between six and seven, would still be small. It is the disfigurement and the confusion they produce in the diagram which limits the usefulness of the instrument to cases in which they can be avoided.

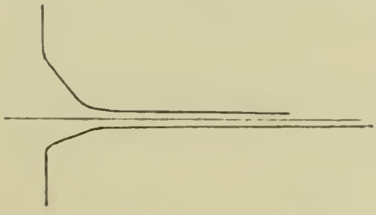
So long as there are thirty of these oscillations in a cycle the necessary fluid friction of the indicator-piston will so far reduce them as to render a fair diagram possible, but when the number approaches fifteen it becomes necessary to call in the aid of considerable pencil-pressure to prevent their destroying the form of the diagram; and when n is as low as ten it is all the pencil will do to prevent them upsetting the diagram. The Author has never been able to produce a respectable diagram when the number is as low as ten, but accounts are continually published in which from the speed of the engine and strength of the springs the value of n must be below this. In such cases the pressure of the pencil must have been very great, and it becomes a question how far this cure is a less evil than the disease.

(2.) *The Friction arising from the Pressure of the Pencil.*—This always acts to oppose the motion of the pencil, and therefore renders it too large during expansion and exhaust, and too small during compression and admission, and thus the general effect is to increase the size of the diagram.

In order to understand this effect, it is necessary to notice that this friction consists of two parts: (1) That of the pencil on the paper. (2) That of the mechanism, caused by sustaining the pressure of the pencil.

The effect of the actual friction of the pencil is greatly reduced by the motion of the paper. Thus, if while the drum is at rest, the pencil be lifted quietly it will be possible for friction to hold it above or below the atmospheric-line, by a distance depending on the pressure. If, when placed as high or low as it will stand, the drum be moved by the cord, the pencil at once approaches the atmospheric-line, describing a line as shown in Fig. 2 at first sloping toward the atmospheric-line at 45°, but finally becoming parallel. Fig. 2 represents the results with a 20-lb. spring; the distance at starting was equal to about 4 lbs., but eventually became about ½ lb., at which it remained constant.

FIG. 2.

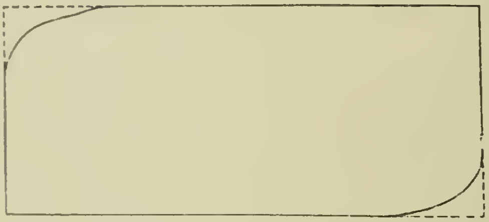


The distance at starting represents the extreme friction of pencil and mechanism. The final distance that of the mechanism alone.

These effects on the diagram are different. That of the pencil causes the pencil to be behind its true position, by a quantity which will bear to the extreme distance a ratio equal to the sine of the inclination of the curve it is describing at the instant to the atmospheric-line.

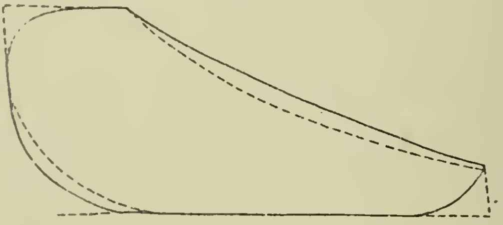
The effect of this alone on a rectangular diagram would be to round off the corners as in Fig. 3.

FIG. 3.



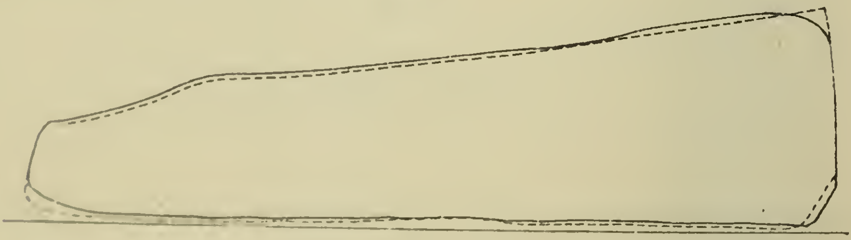
With an early cut-off, the effect would be as shown in Fig. 4.

FIG. 4.



The friction of the mechanism causes the pencil to be behind its true position by a nearly constant quantity, and hence during expansion and exhaust the pencil will be too high, and during compression and admission the pencil will be too low. This is shown in Fig. 5. Its effect on the area of the diagram is therefore not very great.

FIG. 5.



The magnitude of these effects taken together on the area of the diagram depends on the construction of the instrument and on pencil-pressure. From numerous experiments with Richards' and Thomson's indicators, it was found that a comparatively slight alteration of pencil-pressure from that just sufficient to mark the diagram, would cause an excess of 0·5 lb. during expansion, and an equal fall during compression. While if pencil-pressure were made sufficient to prevent serious oscillations when $n = 15$, the mean acting pressure was affected by as much as 1·5 lb. Thus it would appear possible to make a difference of as much as 5 per cent. in a locomotive in mid-gear by pencil-friction.

The conclusions, then, as regards the motion of the pencil, are, that the general effect of inertia and friction are both to increase the size of the diagram; that so long as the speeds are such that n is not greater than 15, the effect of inertia is less than 1 per cent., but that if n is less than 30, oscillations will show themselves unless the pencil-friction be increased. They may, by this, be kept down till $n = 15$, but not farther, and then the necessary friction will affect the area of the diagram about 5 per cent. A speed, therefore, which makes $n = 15$ is about the limiting speed at which diagrams can be taken accurate to 5 per cent., while for the diagrams to be sensibly accurate and free from oscillation the speeds must not be greater than will make $n = 30$.

These speeds for Richards' indicators are given in Table II.

TABLE II.

e	N	
	$n = 30$	$n = 15$
20	69	138
30	85	170
40	99	198
50	105	210
60	120	240
70	130	260
80	139	278
90	147	294
100	155	310

SECTION III.—DISTURBANCES OF THE DRUM.

These are the disturbances (3), (4), (5), section (1). They arise from the elasticity of the cord and mechanism connecting the drum with the piston of the engine. In order to express them definitely—

l is the indicated length of the diagram in inches;

y the yielding of the mechanism in inches per lb. of the tension.

I the moment of inertia of the drum.

(3.) *The Inertia of the Drum.*—If the obliquity of the connecting-rod of the engine be disregarded, and x be put for the distance O P (Fig. 1), the force arising from inertia is proportional to $N^2 x$

and the disturbance arising from this cause will be $y\,I\,N^2\,x$. And as x will be positive or negative according as P is to the right or left of O, the diagram will be uniformly elongated.

The effect of the obliquity of the connecting-rod would be to increase this elongation at the back-end and diminish it at the front, increasing the area of the back-end diagram, and diminishing that of the front somewhat, but it is small unless the connecting-rod is very short.

(4.) *The effect of the varying stiffness of the Spring.*—Let q be the difference of tension of the spring at the extreme ends of the diagram. Then the disturbance of the point P will be

$$-\frac{q\,y\,x}{l}.$$

This effect is therefore opposite to that of (3), and the joint effect will be

$$\left(I\,N^2 - \frac{q}{l}\right) y\,x,$$

and since $I\,N^2$ will be zero at small speeds, and it increases as the square of the speed, when the speed is low the diagram will be $q\,y$ too short, but as the speed increases this shortening will diminish until at some speed $I\,N^2 = \frac{q}{l}$, and for higher speeds the diagram will be elongated. With the Richards' indicator, the critical speed appears to be 150 = N. In most diagrams these effects are apparent, but, except when the connecting-rod is short, they do not affect the indicated pressure.

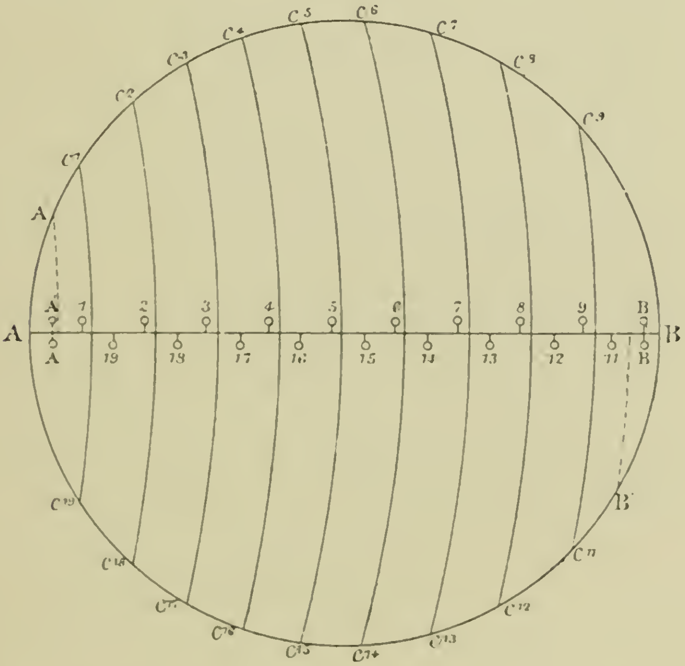
(5.) *The effect of the Friction of the Drum.*—Let F be the tension on the string necessary to overcome the friction of the drum in either direction.

Then during the forward stroke the string will be stretched from this cause $y\,F$, and during the backward stroke it will be shortened $y\,F$. The effect will be to place the drum always behind its true position by $y\,F$. This is shown in Fig. 6.

A $c_1 c_2$ &c. represent the positions of the crank on its circle, as explained in reference to Fig. 1; but in this case $c_1\,c_2$ &c. are chosen so as to correspond with the equidistant positions of the piston. Projecting $c_1\,c_2$ with the connecting-rod as radius on to the atmospheric-line the points are obtained in which, for a true diagram, the pencil would be when the crank was in the positions $c_1\,c_2$ &c., but owing to the cause under consideration, as the crank moves from A towards B, the pencil will be (at the points ♀) at a distance F y behind its true position, and from B to A (at the points ♂) F y behind its true position.

When the crank arrives at A from B the pencil will not as it should arrive at A, but at the point (marked ♂ A) distant F y towards B. This is the end of the indicated stroke, and here the drum will remain until the piston of the engine has reversed its position (with regard to ♂ A), that is, until the crank has reached A′; hence, as the crank moves from A to A′, the drum will be stationary, and then move off distant F y behind its true position, which distance it will maintain until the crank reaches B, when the drum will again rest (at ♀ B) until the crank has reached B′,

FIG. 6.

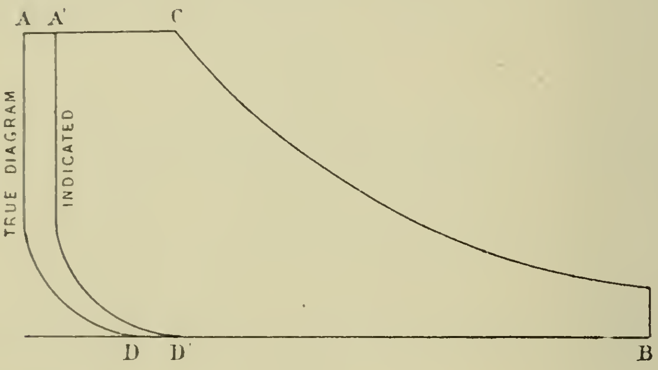


when it will again start towards A distant F y behind its true position.

The effect of this disturbance on a diagram is very great.

In the first place, it must be noticed that supposing y the same, *i.e.*, the length of cord used the same, the effect will be the same on both diagrams. In starting from either end the drum does not move until the engine-piston has moved through a distance F y, and the crank has moved through A A′ or B B′, so that, however the pencil of the indicator may have been moved, in this interval it will merely describe a vertical line (a very common feature of diagrams). For the rest of the motion the drum will move at a constant distance behind its true position, so that the two halves of the diagram will be of the right shape, but wrongly placed with regard to each other. If, then, the pressure at the ends of the true diagram rose and fell instantaneously, so that the extreme ends are vertical, as shown by the line A C B D in Fig. 7, the indicated diagram A′ C B D′ would be obtained from the true diagram by simply giving a horizontal shift (as in Fig. 7) A A′ = 2 F y to the lower half of the diagram-line A D B.

FIG. 7.



The apparent cut-off is then shortened by—

$$A\,A' = 2\,y\,F \quad . \ . \ . \quad . \ . \ . \ . \ . \ . \qquad (6)$$

The diagram is shortened 2 y F.

The area is diminished by—

$$\frac{p_1 - p_3}{e}\,A\,A';$$

and putting $i_m = \text{area}\,\frac{c}{l}$

The effect f on p_m, or $p_m = i_m + f$, is given by—

$$f = (i_1 - i_m - i_3)\,\frac{2\,F\,y}{l + 2\,F\,y} \quad . \ . \ . \ . \ . \ . \ . \qquad (7)$$

It is thus seen that f increases with the expansion and compression, and is zero when these are zero.

This effect of the friction of the drum appears to be so important, and to have been so entirely unperceived, that it may be well to introduce a short discussion of the circumstances on which it depends, and on its effects.

The circumstances are the elasticity of the cord and the friction of the drum, and the important question is, how far these exist in the ordinary indicators? In answer to this, it may be said that the diagrams which led to the discovery of this effect were taken with an indicator which had been in constant use for several years. It was in apparently perfect condition, and the diagrams did not differ essentially from those which had been previously taken. The cord was one which was supplied by the maker. The manner of the discovery was as follows: For years the Author had pursued in the class the method of testing the vibrations of the indicator-pencil by projecting them on to the crank-circle, as shown in Fig. 1, and he had all along noticed that the first oscillation fell short, and shorter in the back-diagram than the front. The cause of this was not obvious, as there seemed to be several possible explanations, and it was partly with a view to determine this cause that Mr. Brightmore's investigation was commenced. A slight error in the reducing-rod, which had a fixed centre and a slot in which a stud in the slide-block worked, was altered at Mr. Brightmore's suggestion. This, however, did not get rid of the effect. A new cord obtained from the makers was substituted for the old one, and the effect was found to be much enhanced, the new cord being more elastic than the old one. This reduced it to the stretching of the cord, but it was only after carefully working out the effect of the inertia of the drum, and it was seen this effect was to lengthen, not shorten, the first oscillation at the back-end, that it occurred to the Author to look to the friction. The indicator was then taken to pieces, cleaned and oiled; then the effect was much reduced. Several new wires and cords were used which gave less effects, and eventually the steel wire was adopted by Mr. Brightmore as the best. The test supplied by the oscillations could only be applied to diagrams taken at high speeds, and the test furnished by the effect upon area was vague. What was wanted was an independent means of determining the simultaneous positions of the drum and the engine-piston. As the best method of meeting this, it was decided to arrange an electric-circuit through the pencil to the drum, with sufficient electromotive force to prick the paper, making the engine-piston close this circuit at eleven definite equidistant points in its motion backwards and forwards. After some difficulty this was successfully carried out by Mr. Brightmore and Mr. Foster. In this way the stretching of the cord during the backward and forward strokes was definitely ascertained by Mr. Brightmore. Taking the smallest results obtained with a cord, it appears from these experiments that the least difference of stretching was to make—

$$2\,F\,y = 0 \cdot 05\,C \text{ in inches} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

where C is the length of the cord in feet; so that there is obtained from equation (7)

$$f = (i_1 - i_m - i_3)\,\frac{0 \cdot 05\,C}{l + 0 \cdot 05\,C} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

This equation gives the value of f or $p_m - i_m$ for any diagram in terms of the length of the cord, on the assumption that the stretching is the same per foot of cord. The length of cord is generally $1 \cdot 5$ time the stroke for the front-end, and $2 \cdot 7$ times for the back-end, or $2 \cdot 1$ for both, hence putting S for the stroke in feet—

$$f = (i_1 - i_m - i_3)\,\frac{0 \cdot 075\,S}{l + 0 \cdot 075\,S} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

for the front-end,

$$f = (i_1 - i_m - i_3)\,\frac{0 \cdot 135\,S}{l + 0 \cdot 135\,S} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (11)$$

for the back-end

or $$f = (i_1 - i_m - i_3)\,\frac{0 \cdot 105\,S}{l + 0 \cdot 105\,S} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (12)$$

as the mean.

In the College engine, with 3 cwt. on the brake, at a speed of one hundred and seven revolutions,

$$S = 1 \cdot 5;$$
$$l = 5 \cdot 0;$$
$$i_1 - i_3 = 30 \cdot 0;$$
$$i_m = 23 \cdot 0.$$

From (12)

$$f = 0 \cdot 24;$$

or

$$\frac{f}{i_m} = 0 \cdot 01.$$

In a locomotive-diagram, Fig. 8, published in "Richards' Indicator," by Porter,

$$S = 2;$$
$$l = 4;$$
$$i_1 - i_3 = 105;$$
$$i_m = 40;$$
$$f = 3 \cdot 25;$$
$$\frac{f}{i_m} = 0 \cdot 085.$$

In the case of a condensing-engine S = 3·5, cutting-off at apparently $\frac{1}{10}$ —

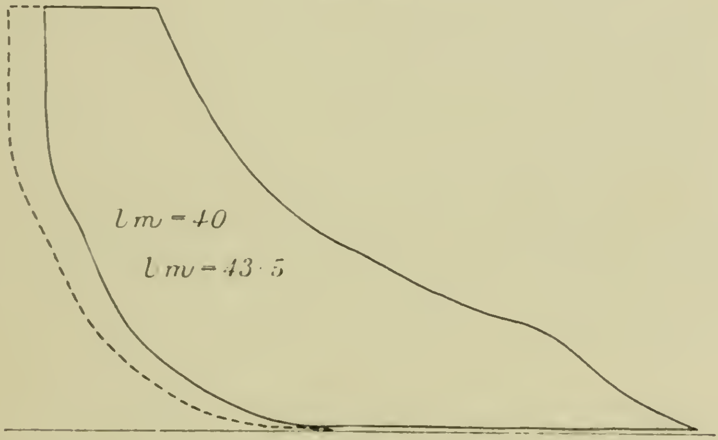
$$\frac{f}{i_m} = 0 \cdot 2;$$

and in the case of a compound-engine expanding ten times—

$$\frac{f}{i_m} = 0 \cdot 10.$$

These would seem to be the smallest results that can have occurred in ordinary practice. The conclusion, however, that hitherto the normal indicated power from engines has been from 10 to 20 per cent. too small is one which must be received with hesitation, or must wait for verification. Yet it may be pointed out that there are not wanting independent evidences of such an effect. There are features common to most diagrams which are shown in this investigation to be due solely to this effect.

FIG. 8.



(i.) In diagrams taken from engines at high speeds the admission-line would not but for this effect be vertical. It would show a certain amount of detail, and the first oscillation would not have a sharp top. They would be as shown in Fig. 9, whereas they commonly are as in Fig. 10.

(ii.) It is commonly found that the expansion-line is above the true expansion-line for the steam allowing for clearance. This fact has been much commented upon, and is sometimes assumed to indicate leaking valves, and sometimes a large amount of evaporation from the jacket, either of which circumstances may explain some rise of the expansion-line towards the end of the stroke, but it is difficult to see how they can explain the rise from cut-off which is usually observed. Now this apparent rise in the curve of expansion is exactly what would result if the apparent cut-off were too early, and this is the result of the effect that has been considered. The Author has tried several diagrams, and he finds that, correcting the cut-off by formula 6, the expansion-line comes out very close indeed to the true curve.

(iii.) In making these comparisons the explanation of another feature of diagrams became apparent. When the two diagrams are traced on the same card there is sometimes seen a want of symmetry about them, and almost invariably when this is the case the cut-off is shorter on the back than on the front-diagram. This would be the result of the friction of the drum, supposing the cord for the back-diagram longer than that for the front. Where this is the case the relative lengths of the cord are about 1 to 1·8.

FIG. 9.

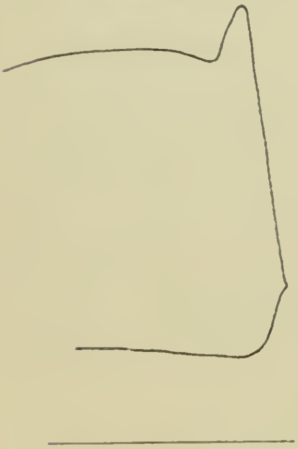
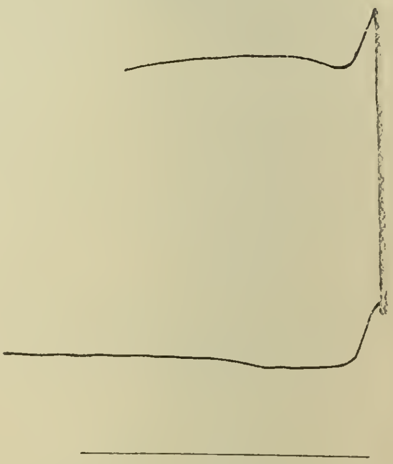


FIG. 10.



These observations are all illustrated in Fig. 11, which represents a facsimile diagram from "Richards' Indicator."

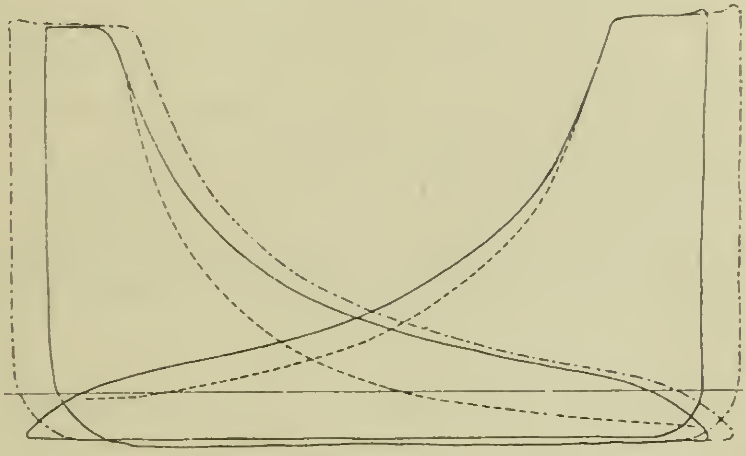
To test this diagram a tracing was taken, and reversed so that the front-diagram was superimposed on the back. It was then observed—

(*a*) That the diagrams were of different lengths, and the difference was about the same as the difference in cut-off.

(*b*) That notwithstanding the apparent cut-off in the back-diagram is to that in the front in the ratio of 2 to 3, the expansion-line of the back-diagram was exactly the same shape as that of the front.

(*c*) That if the diagrams were restored by formula 8, supposing the lengths of cords used to have been 5 feet and 9 feet, the diagrams became exactly similar, and allowing 2 per cent. clearance, the expansion-line comes to be the true expansion-line for that cut-off. This rearrangement is shown in the dotted lines in Fig. 11, the mean pressure from which is 14 per cent. larger than from the original diagrams.

FIG. 11.



Such instances as these seem to sufficiently establish a *primâ facie* case against the confidence which appears to be at present placed in the accuracy of indicator-diagrams. But, in conclusion, the Author would state that he should be very disappointed if anything in this investigation should have the effect of diminishing reliance on the indicator itself. He would have the instrument treated as other instruments have been treated, and instead of its results being assumed accurate, he would have it the object of careful study and experimental investigation, so that the limits of its wonderful perfection may be exactly known, and that reliance placed on it which such knowledge must afford.

The Paper is accompanied by several diagrams, from which the figures in the text have been engraved.

10 November, 1885.

Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

(*Paper No. 2071.*)

"Experiments on the Steam-Engine Indicator."[1]

By Arthur William Brightmore, B. Sc., Stud. Inst. C.E., Late Berkeley Fellow in Owens College, Manchester.

The object of these experiments was to ascertain definitely to what extent certain disturbing causes, which exist in the indicator, affect the diagram.

These disturbing causes are:—

1st. The necessary inaccuracy of the indicator springs, when cold or hot.

2nd. The effect of the inertia of the piston and parallel-motion bars on the area.

3rd. The effect of the oscillations of the spring on the diagram, and the extent to which these may be reduced without sensibly altering its area.

4th. The effect produced by the stretching of the indicator-cord. To get rid, as far as possible, of the error due to this cause, in the experiments relating to the second and third causes, a thin steel wire (B. W. G. 22) was used instead of a cord.

The following is a description of the apparatus employed:—

Indicator.

The indicator was an ordinary Richards indicator, made by Elliott Bros., London; having Watt's parallel-motion for magnifying the deflection of the spring. Springs by different makers were used.

Engine.

The engine employed was the one which is used for the Owens College workshop. It was not chosen on account of any particular adaptability for the purpose; in fact, in some of the experiments, although it fulfilled the requirements, the results were not so marked as they would have been, had the point of cut-off been earlier; but it was necessary in the experiments to have complete control over the engine, and to be able to run it with the brake on only, and the College engine presented these facilities.

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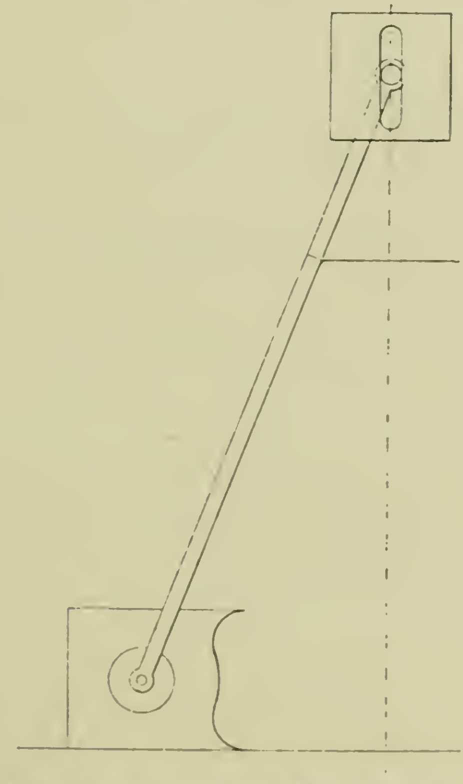
[1] The discussion upon this Paper was taken together with that upon the preceding one.

It is a non-condensing engine, with 9-inch cylinder, 18 inches length of stroke, and a fly-wheel 16 feet in circumference. The point of cut-off is towards the end of the stroke. It works up to a boiler-pressure of about 47 lbs. on the square inch, and to a speed of about 150 revolutions per minute.

REDUCING-MECHANISM.

In order to give the paper-drum a reduced motion of the piston, the wire employed to rotate the drum was attached to a rod, one end of which turned on a pin in the cross-head, and the other end worked in a slot, fixed vertically over the middle position of the cross-head pin, as shown in Fig. 12.

FIG. 12.



MECHANISM FOR REDUCING THE MOTION OF THE PISTON.

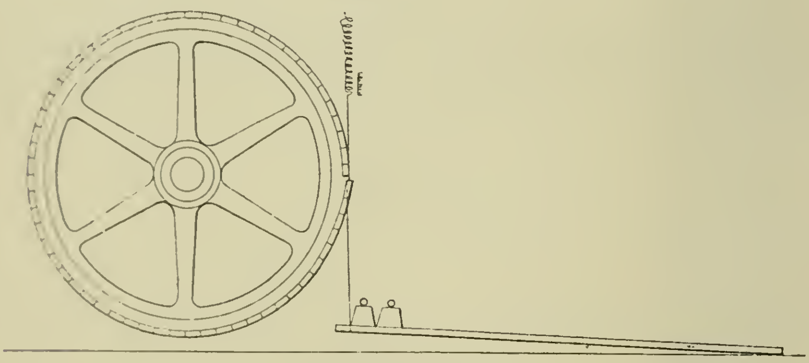
By this method of reducing the motion of the piston of the engine, the only error that comes in is due to the slight change of inclination of the wire.

BRAKE.

The work done was measured by the friction-brake used in the class experiments at Owens College.

It consists of small flat blocks of wood threaded on a cat-gut rope, and is passed round the fly-wheel. To one end of this rope a board, of which the other extremity rests on the ground, is fastened; and the load is placed on the board close to its attachment to the brake. The other end of the brake is attached to a spring-balance, which measures the tension on it; the arrangement is shown in Fig. 13.

FIG. 13.



FRICTION-BRAKE.

Thus, the rate of work was obtained by multiplying the difference of the tensions at the two ends by 16 feet (the circumference of the fly-wheel).

SPEED-INDICATOR (FIG. 14).

This was also a class instrument. It consists of a small paddle-wheel fixed on a vertical axis, in a small circular box containing coloured liquid. Near the bottom of this box, an upright glass tube is inserted. The paddle-wheel was rotated by a cord, driven from a pulley on the main shaft, and passing round a pulley fixed on the same vertical spindle with it.

The rotation of the paddle causes the liquid to rise in the tube to a height dependent on the speed of the engine.

Thus the scale was graduated by running the engine at constant speeds and counting.

INDICATOR-SPRINGS (FIG. 15).

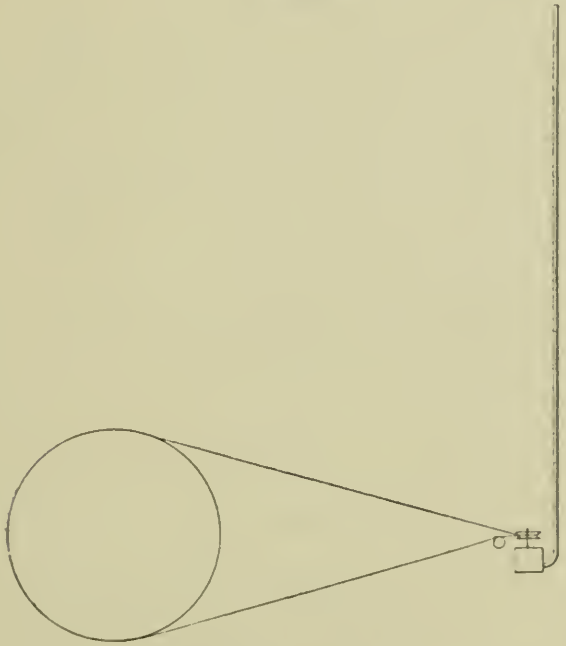
Before commencing the experiments, it was necessary to test the accuracy of the indicator-springs.

To do this, the indicator was rigidly fixed in a vertical position, and pressure was applied to the centre of the indicator-piston by means of a rod, pressed upwards by one end of a long beam, balanced on a knife-edge; the weight being hung on the other end of the beam.

The deflection of the springs was measured by Professor Reynolds' small cathetometer, used in his experiments on "Thermal Transpiration," and fully described in the Philosophical Transactions of the Royal Society, Part II, 1879.

It consists of a microscope carried by a vertical sliding-piece moved by a very accurate screw with fifty threads to the inch, and is capable of measuring to $\frac{1}{10,000}$ inch. Thus, by continually adjusting the screw, so that some well-defined mark on the piston-rod lay on the horizontal cross-hair, and noting the reading for each particular weight, the deflections under the various pressures were arrived at.

Fig. 14.



Speed-Indicator.

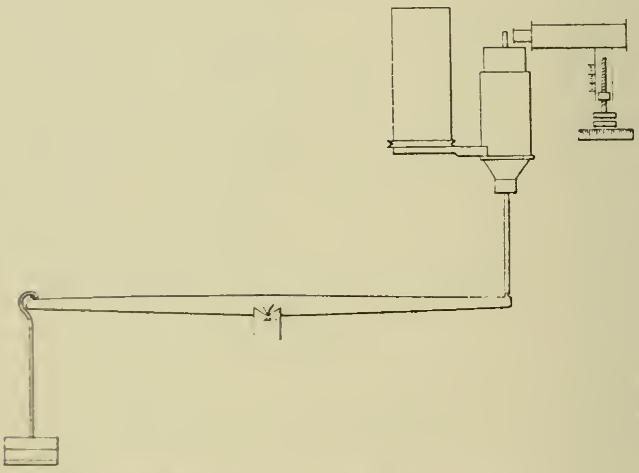
To prevent the piston of the indicator sticking in a wrong position, owing to friction, the frame to which the indicator was attached was tapped with a light hammer each time a fresh weight was added.

Table I gives the results of these experiments for five springs, at the ordinary temperature.

The next thing was to see what effect an increase of temperature would have on the springs. Now, the temperature of the indicator spring never rises above 212° Fahrenheit, owing to its being open to the atmosphere, and moisture always being present in the indicator. Hence the springs were surrounded with steam at 212° Fahrenheit, by passing it through a hole in the cap of the indicator; of course the steam was at first all condensed; but by waiting until steam issued from another hole in the cap, the temperature was maintained uniformly at 212° during the experiments, which were conducted as in the previous cases. The result of these experiments for the same five springs is given in Table II.

Tables I and II show the uniformity of the increase of the deflection with a constant addition to the pressure on the spring.

FIG. 15.



APPARATUS FOR TESTING THE SPRINGS.

They also prove that the deflection of a spring is greater, under the same weight, the higher the temperature; hence the necessity of setting indicator-springs when hot, *i.e.*, when at the temperature of boiling water. It appears also from these Tables, that in the case of the springs experimented upon, the deflection under a given weight at 212° Fahrenheit is about 3 per cent. greater than at the ordinary temperature; therefore a diagram, taken with a spring which is perfectly correct when cold, will be 3 per cent. too large.

This is shown more clearly in Table III, which gives the mean deflection of springs under 1 lb. when cold and when hot, as calculated from Tables I and II, and the deflection under 1 lb. as calculated from the number marked on the spring. The percentage error in the fifth column is the difference between columns three and four, and is allowed for in all the following calculations. It will be noticed from this Table, that in one case only did this error amount to 2 per cent.

TABLE I.—DEFLECTION of SPRINGS, when COLD.

Strength of Spring, lbs. to the Inch.	20		32		32[1]		50		80	
Weight in Scale Pan.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.
	0·6829		0·4887		0·4888		0·4832		0·4791	
		0·0246		0·0156		0·0157		0·0099		0·0069
1	0·7075		0·5043		0·5045		0·4931		0·4860	
		0·0242		0·0156		0·0154		0·0104		0·0061
2	0·7317		0·5199		0·5199		0·5035		0·4921	
		0·0245		0·0154		0·0154		0·0101		0·0063
3	0·7562		0·5353		0·5353		0·5136		0·4984	
		0·0243		0·0153		0·0152		0·0101		0·0062
4	0·7805		0·5506		0·5505		0·5237		0·5046	
		0·0240		0·0156		0·0153		0·0100		0·0062
5	0·8045		0·5662		0·5658		0·5337		0·5108	
		0·0240		0·0154		0·0152		0·0100		0·0062
6	0·8285		0·5816		0·5810		0·5437		0·5170	
		0·0245		0·0153		0·0153		0·0095		0·0062
7	0·8530		0·5969		0·5963		0·5532		0·5232	
		0·0250		0·0157		0·0156		0·0099		0·0062
8	0·8780		0·6126		0·6119		0·5631		0·5294	
		0·0250		0·0157		0·0155		0·0098		0·0062
9	0·9030		0·6283		0·6274		0·5729		0·5356	
		0·0248		0·0157		0·0153		0·0100		0·0061
10	0·9278		0·6440		0·6427		0·5829		0·5417	
		0·0249		0·0155		0·0157		0·0101		0·0060
11	0·9527		0·6595		0·6584		0·5930		0·5477	
		0·0247		0·0155		0·0154		0·0099		0·0060
12	0·9774		0·6750		0·6738		0·6029		0·5537	
		0·0246		0·0158		0·0160		0·0099		0·0059
13	1·0020		0·6908		0·6898		0·6127		0·5596	
		0·0244		0·0156		0·0156		0·0100		0·0060
14	1·0264		0·7064		0·7054		0·6227		0·5656	
		0·0243		0·0158		0·0155		0·0100		0·0061
15	1·0507		0·7222		0·7209		0·6327		0·5717	
		0·0246		0·0156		0·0161		0·0100		0·0059
16	1·0753		0·7378		0·7370		0·6427		0·5776	
		0·0238		0·0157		0·0159		0·0100		0·0061
17	1·0991		0·7535		0·7529		0·6527		0·5837	
		0·0251		0·0156		0·0154		0·0100		0·0059
18	1·1242		0·7691		0·7683		0·6627		0·5896	
		0·0244		0·0150		0·0151		0·0100		0·0062
19	1·1486		0·7841		0·7834		0·6727		0·5958	
		0·0242		0·0152		0·0154		0·0100		0·0072
20	1·1728		0·7993		0·7988		0·6827		0·6030	
		..		0·0152		0·0156		0·0102		0·0062
21	..		0·8145		0·8144		0·6929		0·6092	
		..		0·0154		0·0155		0·0101		0·0062
22	..		0·8299		0·8299		0·7030		0·6154	
		..		0·0156		0·0153		0·0103		0·0061
23	..		0·8455		0 8452		0·7133		0·6215	
		..		0·0151		0·0155		0·0100		0·0063
24	..		0·8606		0·8607		0·7233		0·6278	
		..		0·0152		0·0160		0·0101		0·0062
25	..		0·8758		0·8767		0·7334		0·6340	
		..		..		..		0·0099		0·0062
26	..		..		..		0·7433		0·6402	
		..		..		..		0·0102		0·0066
27	..		..		..		0·8535		0·6468	

[1] Different maker.

TABLE II.—DEFLECTION of SPRINGS, when HOT.

Strength of Spring, lbs. to the Inch.	20		32		32[1]		50		80	
Weight in Scale Pan.	Reading of Micrometer.	Deflection under 1lb.	Reading of Micrometer.	Deflection under 1lb.	Reading of Micrometer.	Deflection under 1lb.	Reading of Micrometer.	Deflection under 1lb.	Reading of Micrometer.	Deflection under 1lb.
	0·6872		0·4941		0·4937		0·4862		0·5953	
		0·0245		0·0154		0·0162		0·0104		0·0063
1	0·7117		0·5095		0·5099		0·4966		0·6016	
		0·0251		0·0160		0·0157		0·0108		0·0065
2	0·7368		0·5255		0·5256		0·5074		0·6081	
		0·0251		0·0158		0·0157		0·0103		0·0064
3	0·7619		0·5413		0·5413		0·5177		0·6145	
		0·0249		0·0159		0·0156		0·0103		0·0062
4	0·7868		0·5572		0·5569		0·5280		0·6207	
		0·0250		0·0157		0·0157		0·0103		0·0063
5	0·8118		0·5729		0·5726		0·5383		0·6270	
		0·0251		0·0162		0·0156		0·0103		0·0063
6	0·8369		0·5891		0·5882		0·5486		0·6333	
		0·0253		0·0156		0·0157		0·0098		0 0063
7	0·8622		0·6047		0·6039		0·5584		0·6396	
		0·0258		0·0162		0·0164		0·0105		0·0064
8	0·8880		0·6209		0·6203		0·5689		0·6460	
		0·0258		0·0163		0·0159		0·0104		0·0066
9	0·9138		0·6372		0·6362		0·5793		0·6526	
		0·0256		0·0160		0·0160		0·0099		0·0062
10	0·9394		0·6532		0·6522		0·5892		0·6588	
		0·0259		0·0163		0·0158		0·0102		0·0062
11	0·9653		0·6695		0·6680		0·5994		0·6650	
		0·0258		0·0160		0·0162		0·0104		0·0064
12	0·9911		0·6855		0·6842		0·6098		0·6714	
		0·0250		0·0162		0·0158		0·0098		0·0066
13	1·0161		0·7017		0·6700		0·6196		0·6780	
		0·0250		0·0159		0·0162		0·0106		0·0065
14	1·0411		0·7176		0·7162		0·6302		0·6845	
		0·0246		0·0163		0·0163		0·0105		0·0062
15	1·0657		0·7339		0·7325		0·6407		0·6907	
		0·0259		0·0164		0·0161		0·0101		0·0063
16	1·0916		0·7503		0·7486		0·6508		0·6970	
		0·0258		(2)		0·0161		0·0104		0·0067
17	1·1174		0·7689		0·7647		0·6612		0·7037	
		0·0252		0·0160		0·0158		0·0105		0·0064
18	1·1426		0·7819		0·7805		0·6717		0·7101	
		0·0242		0·0159		0·0157		0·0105		0·0061
19	1·1668		0·8008		0·7962		0·6822		0·7162	
		..		0·0159		0·0160		0·0106		0·0065
20	..		0·8167		0·8122		0·6928		0·7227	
		..		0·0159		0·0157		0·0102		0·0062
21	..		0·8326		0·8279		0·7030		0·7289	
		..		0·0163		0·0170		0·0105		0·0063
22	..		0·8489		0·8449		0·7135		0·7352	
		..		0·0162		0·0156		..		0·0064
23	..		0·8651		0·8605		..		0·7416	
		..		0·0159		0·0160		..		0·0063
24	..		0·8810		0·8765		..		0·7479	
		..		0·0162		0·0164		..		0·0063
25	..		0·8972		0·8929		..		0·7542	
		..		..		..		..		0·0060
26									0·7602	

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[1] Different maker.

[2] Condensed steam let out of cylinder

EFFECT OF INERTIA OF THE MOVING-PARTS ON THE AREA OF THE DIAGRAM.

Having ascertained the errors in the springs, the next question was to find how far the effect of inertia tends to alter the area of the diagram before the oscillations appear. To do this, diagrams were taken at various speeds and with several springs. In Table IV the efficiencies, *i.e.*, the ratios of the brake-pressures to the mean diagram-pressures, are given at the various speeds, instead of the mean pressures as calculated from the diagrams, on account of the difficulty of keeping the load on the brake exactly constant.

TABLE III.—MEAN DEFLECTIONS of SPRINGS under 1 lb.

Spring.	Experimental Deflection, cold.	Experimental Deflection, hot.	Deflection from Mark on Spring.	Percentage Error.
	Inch.	Inch.	Inch.	
20	0·0245	0·02525	0·02523	0·08
32	0·0155	0·01600	0·01580	1·25
32[1]	0·0155	0·01595	0·01580	0·95
50	0·0100	0·01030	0·01009	2·08
80	0·0062	0·00636	0·00630	0·94

[1] Different maker.

Now if the inertia affects the areas of the diagrams, the areas of the diagrams, and hence the mean diagram-pressures, will vary directly with the velocity, and inversely as the stiffness of the spring (the weight on the brake being constant); *i.e.*, the efficiencies will vary directly with the stiffness of the spring and with the inverse of the velocity. However, an examination of the Table shows no appreciable increase of the efficiency with greater stiffness of the spring, and no more decrease, as the velocity increases, than would be accounted for by the greater friction.

TABLE IV.

Speed.	Efficiencies.				
	Spring.				Mean Values.
	20	32	50	80	
44	0·94	..	0·95	..	0·945
68	0·93	0·94	0·93	..	0·933
84	..	0·93	0·93	0·93	0·930
107	..	0·93	0·94	0·93	0·933
127	..	..	0·93	0·92	0·925

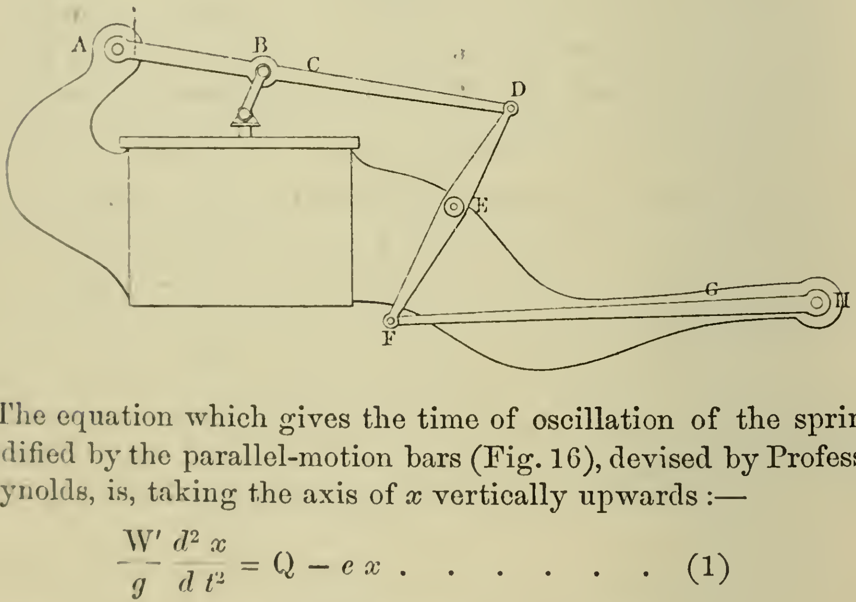
Table IV is not filled in for the 20 and 32 springs at the higher speeds, because the oscillations begin to come in.

The inference is, "that in a given engine, when the ratio of the speed to the stiffness of the spring, used to indicate it, is not so great as to cause oscillations to appear in the diagram, the area is not appreciably affected by the momentum of the moving parts." This seems natural, for, after the initial disturbance on the admission of the steam to the cylinder, the motion of the spring is gradual, and hence its deflection would correspond to the pressure on it.

Oscillations.

When the ratio of the speed of the engine to the stiffness of the spring, used to indicate it, exceeds a certain value, which is different for different engines, oscillations appear in the diagram.

Fig. 16.



The equation which gives the time of oscillation of the spring, modified by the parallel-motion bars (Fig. 16), devised by Professor Reynolds, is, taking the axis of x vertically upwards :—

$$\frac{W'}{g}\frac{d^2 x}{d t^2} = Q - e x \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where

$$W' = W + (w + w_2)\frac{k^2}{a^2} + 16\, w_1$$

$$Q = P - \left(W + (w + w_2)\frac{b}{a} + 4\, w_1\right),$$

and

e = the stiffness of spring.

W = weight of piston + $\frac{1}{2}$ weight of spring.
w = weight of rod A D (Fig. 16).
w_1 = weight of rod D F.
w_2 = weight of rod F H.

P = whole pressure of steam on the piston.
a = A B.
b = A C = G H = distance of centres of gravity of rods A D, F H from A and H respectively.
k = radius of gyration of A D, F H, about A and H respectively.

W′ is, in fact, the weight which would have to oscillate at B to be equivalent to the moving-parts, and the expression P − Q represents the force which would have to be applied at B, if the parts referred to were removed, to be equivalent to them.

Equation (1) is of the well-known form for finding the time of a complete oscillation (T), and then is obtained in the ordinary way—

$$T = 2\,\pi\sqrt{\frac{W'}{g\cdot e}},$$

Or calling N the number of oscillations per minute—

$$N = \frac{30}{\pi}\sqrt{\frac{g\cdot e}{W'}}.$$

It will be noticed that in equation (1), the rotation of the rod DF, which is very slight, is neglected, as also is the friction of the instrument.

In the case of the indicator employed, the values of the above constants were—

$W_{20} = 0{\cdot}10529$ lb. $W_{32} = 0{\cdot}10954$ lb.
$w = 0{\cdot}00957$ lb.
$w_1 = 0{\cdot}01037$ lb.
$w_2 = 0{\cdot}00866$ lb.
$a = 0{\cdot}75$ inch.
$b = 1$ inch.
$k^2 = 1{\cdot}83$.

Whence from the above $W'_{20} = 0{\cdot}33063$ lb.
$W'_{32} = 0{\cdot}33488$ lb.

and from preceding experiments $e_{20} = 475$.
$e_{32} = 750$.

NOTE.—The suffixes 20 and 32 refer to the springs marked 20 and 32 respectively.

Thus—

$N_{20} = 2{,}050$.
$N_{32} = 2{,}560$.

It will be noticed on substituting for W′, that the rod DF has as much influence in causing the oscillations to come in as all the other moving parts together.

To verify these results, diagrams were taken with weak springs, in order to bring in oscillations. It must be understood that the diagrams in this Paper are not intended as specimens of good diagrams, but are merely to illustrate the various points considered.

The time of oscillation of the indicator-springs may be approximately obtained from such diagrams in the following manner:—first, project the crests and hollows of the oscillations vertically down on to the atmospheric line; next, with a radius equal to the length of the connecting-rod (reduced to the same scale as the length of the diagram), and centre on the atmospheric-line produced, project the points so obtained upon a circle described on the atmospheric-line with the length of diagram as the diameter; then the arcs of the circle intercepted between alternate intersections represent the angle turned through by the crank during the time of a complete oscillation of the spring. Hence, assuming that the crank-shaft rotates uniformly, these arcs would represent the time of a complete oscillation.

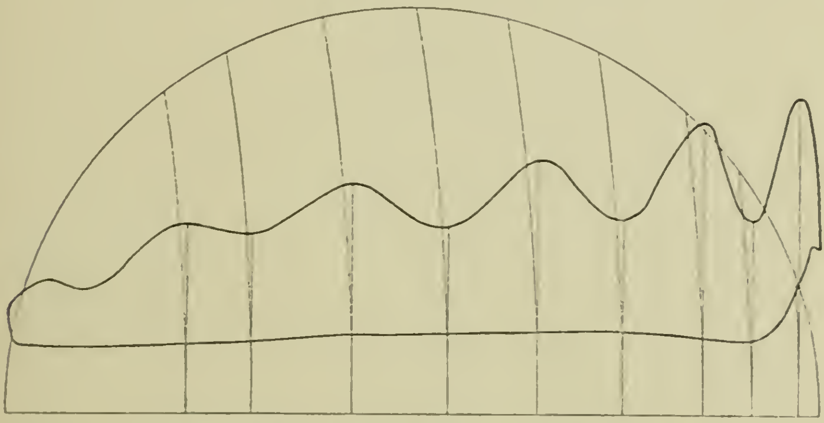
There are several reasons why the number of oscillations per minute so obtained should not quite equal the number as obtained above from theory. Firstly, the neglect of the rotation of the bar D F, and of the friction in the equation, would make a slight difference; but the most important reason is the gradual decrease of pressure in the cylinder of the engine, consequent upon the motion of the piston and initial condensation. This diminution of pressure causes the crests to lie behind, and the hollows to be in advance of their true position (Figs. 17 to 25), by an amount varying with the rate of decrease. Supposing for the moment the lag to be equal in amount for each crest, the projection of it (the lag) upon the crank-circle will include a greater arc towards the ends than in the middle of the diagram; thus, other things being the same, causing the time of oscillation to appear too great at one end of the diagram, and too small at the other end of the diagram. However, this tendency is counteracted, at least during the first half of the stroke (and it is during this period chiefly that the time of oscillation is measured), by the retardation of the velocity of oscillation, and consequently the greater effect of the reduction of pressure in causing the crests to lag as the stroke progresses. That the velocity of oscillation decreases with the distance from the point of admission is seen by integrating equation (1), where—

$$\frac{dx}{dt} = \left(\frac{Q}{e} - c\right) \cdot \sqrt{\frac{ge}{W'}} \cdot \sin \sqrt{\frac{ge}{W'}} \cdot t \quad . \quad (2)$$

where $c = \frac{1}{4}$ the distance of a hollow from the atmospheric line.

Now $2\left(\frac{Q}{e} - c\right)$ is equal to the range of oscillation, as may be seen by again integrating equation (1), and in the case of the diagrams referred to, the range of oscillation, and hence from above, the velocity of oscillation of the spring diminished as the stroke advances, which is almost self-evident, for the time of oscillation is independent of the range, so that if the range be reduced the velocity must be reduced also.

FIG. 17.

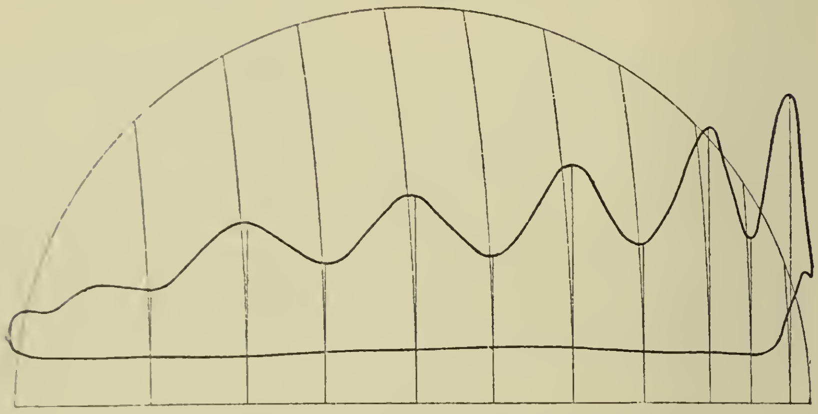


Front end diagram taken with 20 springs at 144 revolutions.

From equation (2) it is also seen that, other things being the same, the number of oscillations in a diagram increases with the stiffness of the spring, hence the counteracting effect, just referred to, would be less marked as the stiffness of the spring used is increased, so that for this reason the number of oscillations per minute as obtained from a diagram would be nearer the truth the weaker the spring.

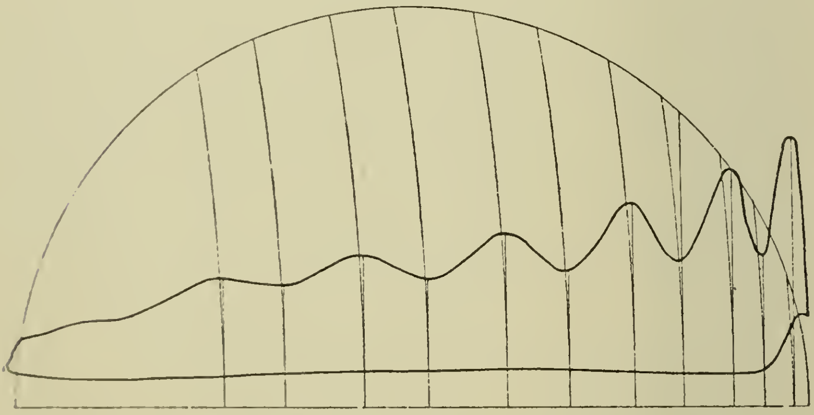
Again, the number of oscillations per minute will probably be nearer the truth the greater the speed of the engine; for the number of oscillations in a diagram is smaller the greater the speed of the engine, because the time of oscillation of the spring is independent of the speed of the engine, and hence the ratio of the velocity of oscillation to the rate of reduction of pressure is less the higher the speed of the engine, hence the counteracting effect referred to is greater. These two points are illustrated in the diagrams, Figs. 17 to 25, and the accompanying Table V.

FIG. 18.



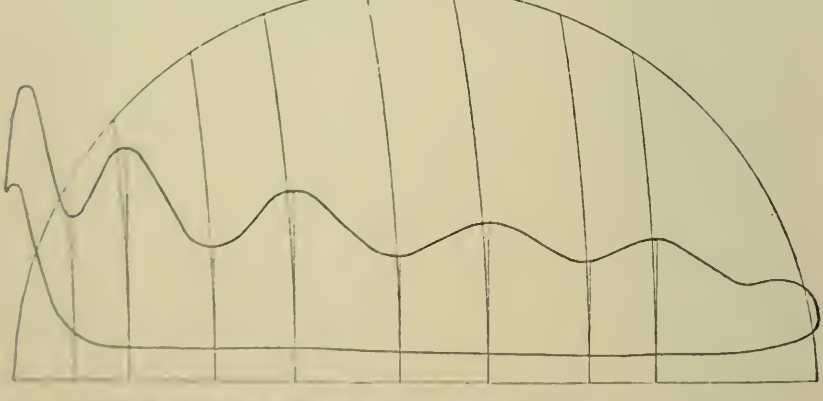
Front-end diagram taken with 20 springs at 127 revolutions.

FIG. 19.



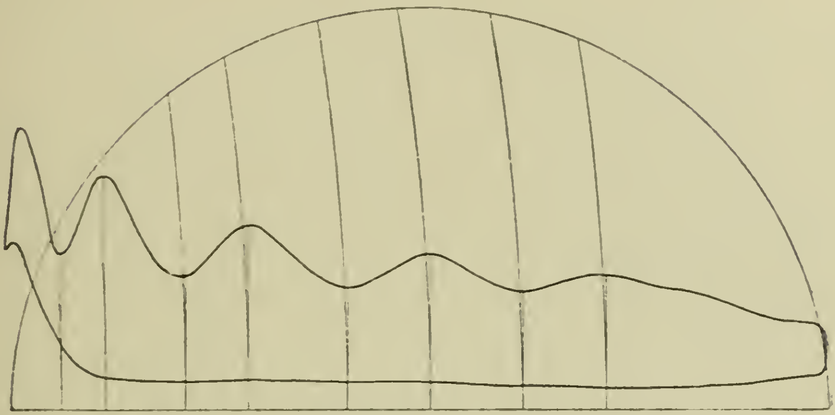
Front-end diagram taken with 20 springs at 107 revolutions.

FIG. 20.



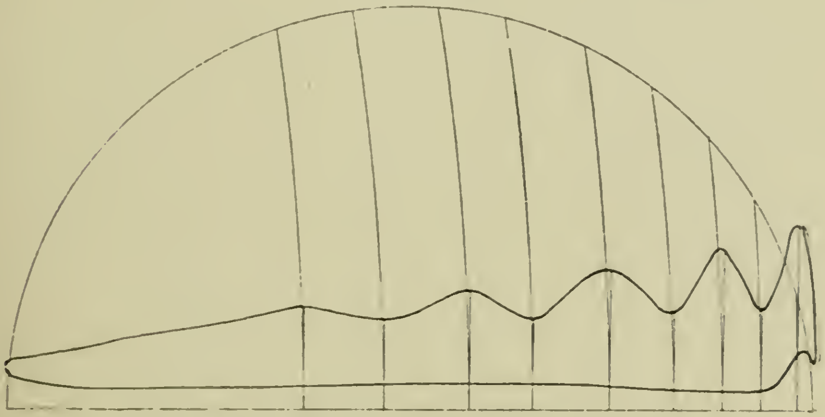
Back end diagram taken with 20 springs at 144 revolutions.

FIG. 21.



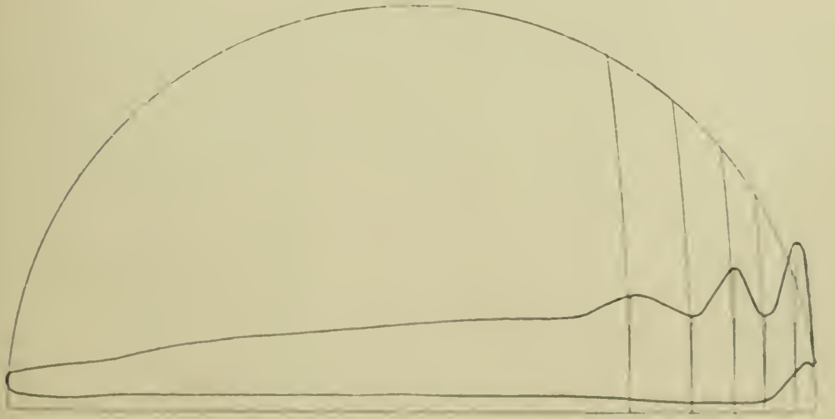
Back-end diagram taken with 20 springs at 127 revolutions.

FIG. 22.



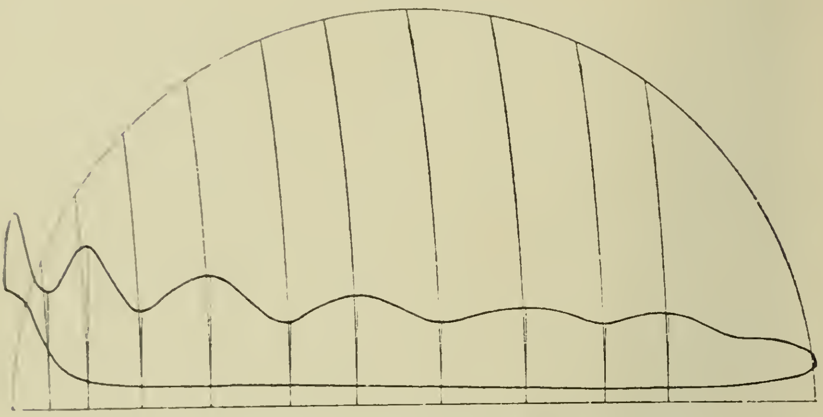
Front-end diagram taken with 32 springs at 144 revolutions.

FIG. 23.



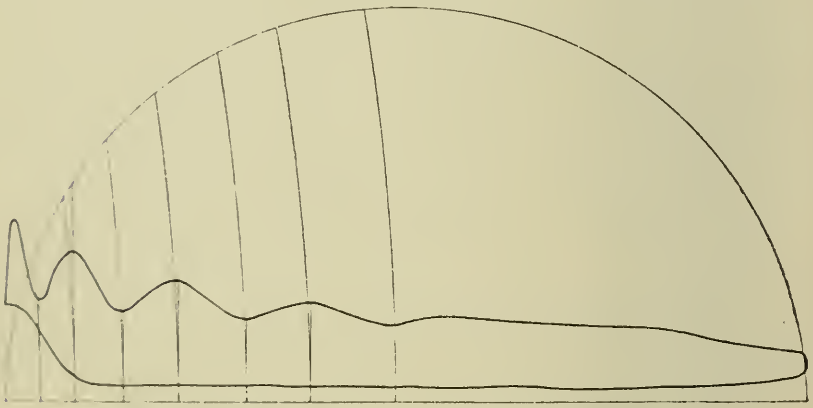
Front-end diagram taken with 32 springs at 127 revolutions.

Fig. 24.



Back-end diagram taken with 32 springs at 144 revolutions.

Fig. 25.



Back-end diagram taken with 32 springs at 127 revolutions.

Table V.

Speed. Revolutions per minute.	End.	Spring.	Number of Oscillations.		Difference per cent.
			From Diagram.	From Formula.	
144 (Fig. 17)	Front	20	1,950	2,050	5·0
127 (Fig. 18)	,,	20	1,920	,,	6·5
107 (Fig. 19)	,,	20	1,883	,,	8·5
144 (Fig. 20)	Back	20	1,950	,,	5·0
127 (Fig. 21)	,,	20	1,930	,,	6·0
144 (Fig. 22)	Front	32	2,370	2,560	7·5
127 (Fig. 23)	,,	32	2,300	,,	10·0
144 (Fig. 24)	Back	32	2,300	,,	10·0
127 (Fig. 25)	,,	32	2,300	,,	10·0

In calculating the oscillations from the diagrams a mean value was taken.

The distance to which the oscillations extend depends on the range of the first one, and on the friction of the pencil. The range of the first oscillation is great if the period of a semi-oscillation nearly coincides with the time the steam takes to attain its maximum pressure on admission; this happens when the engine is running fast. It is small when the time of attaining the greatest pressure of steam and the time of a semi-oscillation are not nearly equal. Thus, when the steam is wire-drawn on entering the cylinder of an engine, that engine would give a better diagram at high speeds than if this were not the case.

Again, if the steam be throttled on entering the indicator, the time of the steam attaining its maximum pressure in the indicator-cylinder will be lengthened; hence the extent of the first oscillation will be reduced, and therefore the oscillations in the diagram will be reduced; but the diagram so obtained does not give a correct idea of the work done, but is too small, in proportion to the amount of throttling.

The effect of the friction of the pencil in lessening the extent of the oscillations varies with the pressure on the pencil. When the oscillations are thus reduced by pressing the pencil on the paper an indefiniteness is introduced into the results, owing to the pencil sticking either too high or too low, and the results cannot be relied on.

To illustrate this point diagrams were taken under the same conditions, of which the results are given in Table VI.

In the case of the weaker springs, 20 lbs. and 32 lbs., the pencil was pressed on the diagram-paper so as to reduce the oscillations. Diagrams were taken with stiffer springs, in which oscillations do not perceptibly enter, to check the results so obtained.

TABLE VI.—FRONT-END EFFICIENCIES.

Speed.	20 Spring (pencil pressed).	32 Spring (pencil pressed).	50 Spring.	80 Spring.
69	0·932	0·927	0·959	0·958
87	0·931	0·942	0·954	0·951
108	0·918	0·907	0·955	0·954
Mean efficiencies	0·927	0·925	0·956	0·955

Table VI shows that in those experiments in which the pencil was pressed on to the paper the results are too small by more than 3 per cent. No doubt if the engine had cut-off earlier, and been working with a higher pressure of steam, the results would have been still more discordant.

Probably the most accurate method of arriving at the mean pressure when the oscillations extend a good way into the diagram, at least when the cut-off occurs late in the stroke as in the present case, is to draw a line midway between the crests and hollows, and to the value for the mean pressure obtained by taking this line add an amount, which in the case of indicators similar to the one employed in these experiments is 0·35 lb.

To see the reason for this, referring back to equation (1), and integrating it twice—

$$x = \frac{Q}{e} + \left(c - \frac{Q}{e}\right) \cos \sqrt{\frac{g\,e}{W'}} \cdot t.$$

Substituting in this $t = \pi \sqrt{\frac{W'}{g\,e}}$ (time of half oscillation)—

$$x = \frac{2\,Q}{e} - c,$$

i.e., Q is the arithmetical mean of $e\,x$ and $e\,c$.

Substituting in this expression the value for Q, and taking the area of the indicator piston as 0·5 square inch, the following value for the intensity of pressure (p) is obtained:—

$$p = e\,(x + c) + 2\left(W + (w + w_2)\,\frac{b}{a} + 4\,w_1\right).$$

Hence if a line midway between the crests and hollows be taken as representing the pressure, the mean pressure so obtained will be too small by the amount of the second term on the right, which for the indicator employed = 0·35 lb. This would be negligible for any considerable pressure.

It was found that with the indicator used, a diagram tolerably free from oscillations could be taken from the engine up to a speed of about 90 revolutions per minute, with a spring of 20 lbs. to the inch. Hence, since the time the steam takes to attain its maximum pressure in the cylinder varies with the speed of the engine (in different engines it would also vary with the arrangement of the slide-valve), it might be expected to obtain a diagram tolerably free from oscillations at a speed of from

400 to 500 revolutions per minute, with an indicator having a parallel-motion in which the rod corresponding to D F is absent, and in which the other moving-parts are as light again as in the present case. This would be the case with an indicator of smaller diameter, in which a much stronger spring could be used for the same weight. For much higher speeds than this, unless the relative time occupied in attaining the maximum pressure increased with the speed, it would appear that the diagrams would be effected to a great but unknown extent by the oscillations of the spring.

VITIATION OF THE DIAGRAM BY THE STRETCHING OF THE INDICATOR-CORD.

The effect of the stretching of the cord varies greatly with the shape of the diagram, and with the state of lubrication of the paper-drum. Owing to the late cut-off, the engine employed in the experiments was not well suited for showing this effect. However, in some experiments, when the paper-drum wanted oiling, the diagram given with the cord was more than 7 per cent. smaller than that given with the steel wire. The effect is in all cases to reduce the area, though not necessarily to reduce the mean pressure calculated from it.

To ascertain if the diagrams from the engine in question would show much difference when taken with cord and with wire, the experiments summarised in Table VII were made. The lengths and efficiencies given are the mean of the front- and back-end diagrams.

TABLE VII.

Speed.	Wire.		String.	
	Length.	Efficiency.	Length.	Efficiency.
	Inches.		Inches.	
68	5·11	0·93	4·78	0·94
84	5·11	0·93	4·80	0·94
107	5·13	0·94	4·80	0·94
127	5·12	0·93	4·80	0·97

Although the efficiency as calculated from the two sets of diagrams is inconsiderable, yet the difference in their lengths points to a large difference in their areas.

The difference in the tension of the indicator-cord at various parts of the stroke may be shown by considering the equation of motion of the indicator-drum.

This equation during the outward stroke is

$$\frac{I\,d^2\,\delta}{d\,t^2} = T\,a - M_s - M_f.$$

where I = the moment of inertia of the drum about its axis.
T = the tension in the cord.
a = radius of drum.
M_s = moment of resistance of the drum-spring about the axis of drum.
M_f = moment of friction about the same line.

Hence $$T = \frac{1}{a}\left(I\frac{d^2\,\delta}{d\,t^2} + M_s + M_f\right).$$

$\frac{d^2\,\delta}{d\,t^2}$, the angular acceleration of the drum about its axis, is a maximum to begin with, and continues to decrease during the stroke, becoming zero near the middle of the stroke.

M_s is constant during the stroke.

M_f is a maximum on starting, then suddenly decreases and then varies directly with some power of the velocity, increasing therefore until about the middle of the stroke, and then diminishing.

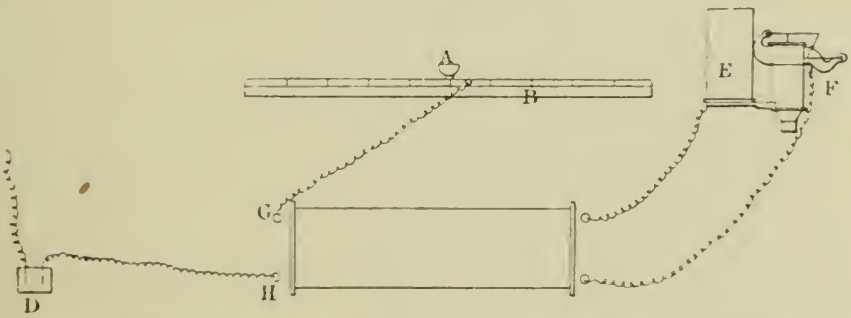
Thus it is evident that during the outward stroke the tension T is a maximum to begin with, decreases rapidly about the middle of the stroke, and more slowly towards the end.

At the end of the stroke the friction suddenly changes sign, thus causing a sudden diminution in the tension at the commencement of the inward stroke; afterwards the tension increases rapidly about the middle of the stroke, and more slowly towards the end.

Hence it might be expected that that part of a diagram taken during the outward stroke would be shortened to commence with, then slightly stretched, and slightly shortened at the end; and that that part taken during the inward stroke, would be first shortened, then lengthened a little, and slightly shortened towards the end, almost as in the case of the outward stroke.

To show that this actually takes place, an arrangement was devised by Professor Reynolds, the object of which was to prick holes in the diagram corresponding to eleven equidistant positions of the piston. For this purpose a Grove battery (D Fig. 26) of five cells, in conjunction with a Ruhmkorff coil, was used. But in order to get the holes pricked in their proper positions, instead of the ordinary arrangement for making and breaking contact, the following plan was adopted, the hammer of the coil being held back. The wire from one pole of the battery was connected with one of the binding-screws (H) of the primary coil as usual, but the wire from the other pole of the battery was connected with the engine. A wire from the other binding-screw (G) of the primary coil was attached to the contact-breaker (B). This consisted of a smooth piece of wood, into which eleven pieces of wire were inserted at equal distances, and filed level with the wood, the distance between the first and the last wires being the length of the stroke of the engine. The contact-breaker was fixed on the lower slide bar, so that the central wire should be at the middle of the stroke, and so that a pointer (A), which was secured to the cross-head, should slide on the smooth piece of wood. Hence every time the pointer crossed a wire on the contact-breaker the circuit of the primary current was complete, and a spark of the induced current passed through the diagram-paper. To bring this about one wire of the induced current was connected with the metallic drum (E), and the other to a cup of mercury (F), into which the metallic pencil dipped, thus completing the circuit of the induced current when the pencil touched the paper.

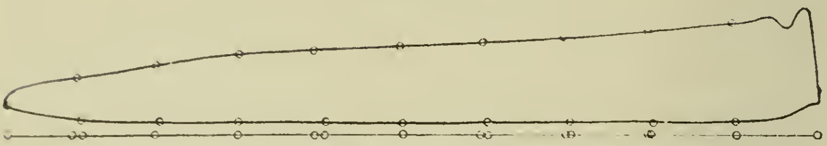
Fig. 26.



Electrical Apparatus for showing the Distortion of a Diagram by the Indicator-Cord.

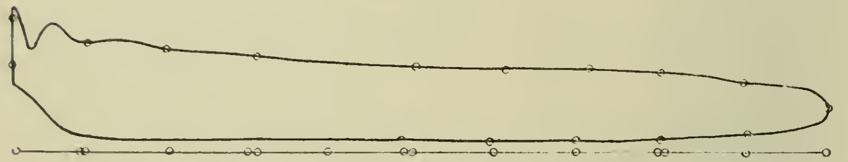
In the diagrams, Figs. 27 to 34, which were taken in this manner, the positions of the pricked holes, corresponding to the eleven equidistant positions of the piston, are indicated by small circles. The relative positions of these circles show which parts of the diagrams are lengthened, and which are shortened. An examination shows that the effect is not merely to shorten the

FIG. 27.



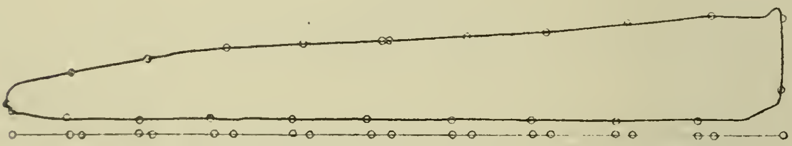
Front-end pricked diagram taken with wire at 107 revolutions.

FIG. 28.



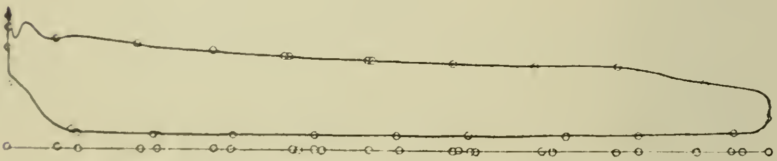
Back-end pricked diagram taken with wire at 107 revolutions.

FIG. 29.



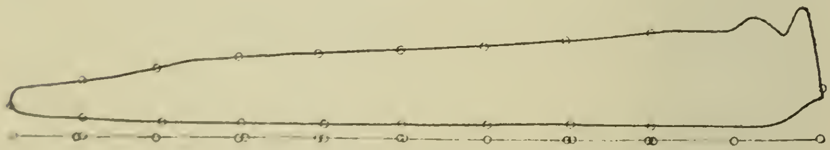
Front-end pricked diagram taken with string at 107 revolutions.

FIG. 30.



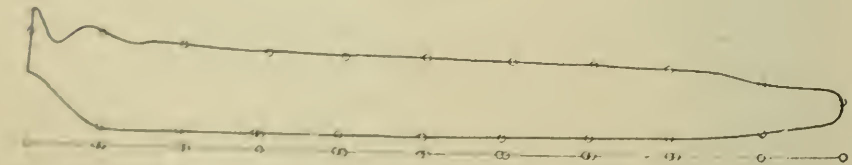
Back-end pricked diagram taken with string at 107 revolutions.

FIG. 31.



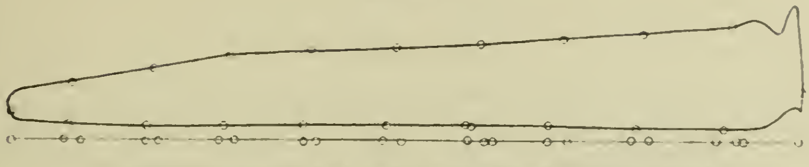
Front-end pricked diagram taken with wire at 127 revolutions.

FIG. 32.



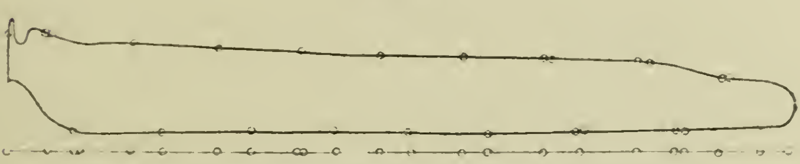
Back-end pricked diagram taken with wire at 127 revolutions.

FIG. 33.



Front-end pricked diagram taken with string at 127 revolutions.

FIG. 34.



Back-end pricked diagram taken with string at 127 revolutions.

ends and lengthen the middle of the diagrams, but also to distort them, *i.e.*, to cause corresponding points in their upper and lower parts not to lie in the same vertical line. The amount of this distortion is shown by the distance between corresponding points on the atmospheric-line. It will also be noticed that even in the diagrams taken with wire instead of with cord this distortion is not altogether absent. The indefiniteness in the stretching of the cord is shown by some of the points being marked twice.

In high-speed diagrams of short length these effects would cause a marked modification in their form when taken with cord.

At high speeds, when the spring of the drum is not stiff enough to keep the cord tight near the centre of the stroke, and the velocity is greatest, a shortening of the middle portion of the diagram, taken during the inward stroke, and a lengthening of the end, would result.

These considerations show that in indicators intended to take diagrams from engines running at high speeds, the drum, as well as all the other moving-parts, should be as light as possible.

The Paper is accompanied by several illustrations and diagrams, from which the figures in the text have been engraved.

Discussion.

Professor Reynolds.

Professor Osborne Reynolds said, as it was five months since his Paper had been read, it might be convenient to refer more fully to the principal points raised than had been done by Mr. Brightmore, whose work had really been the foundation of the Paper. They had gone over the subject in a necessarily more or less systematic manner.

With regard to the oscillations, most persons who had had to do with indicators knew that these constituted a practical limit to the speed of an engine with which a particular indicator, with a particular kind of spring, could be used. The actual limits were shown in Table II (p. 11). The limit of speed was given for a Richards indicator, and every other indicator would have its own limit, which would be higher or lower according to the construction. The limit of speed was given at which a spring of a certain number of lbs. to the inch could be used. One column might be taken as the limit at which the inaccuracy was 5 per cent., and in another it was inappreciable. The result of that friction, according to the amount of pressure on the paper, was shown in Mr. Brightmore's experiments to amount to 3 per cent. on the mean pressure area of the diagram, diminished by the efficiency of the engine as measured by the brake. There was apt to be a little confusion with regard to that point. The effect of the increase of the area of the diagram in Mr. Brightmore's experiment was to diminish the efficiency of the engine, because the efficiency was the brake-power divided by the indicated-power. The greater the indicated-power, the smaller the inefficiency; consequently the effects were 3 per cent. too low when the pencil was pressed home. The pressure of the pencil, beyond what was sufficient to trace the diagram, was unnecessary, except to destroy the oscillations. When the engine was working slowly, it was not necessary to press the pencil home, but with an engine running at high speed, and the oscillations going on, the only way to destroy them was to press the pencil home. The continuous line in the diagram, Fig. 5, represented the actual position when the pencil was pressed, and the dotted line the true position, or the actual position with very light pressure. It would be seen that the pencil was above the true position all the way during expansion and exhaust, and below the true position during compression and admission. The result was to increase the area

Professor Reynolds.

of the diagram, and Mr. Brightmore's experiment showed about 3 per cent. in a nearly parallel diagram. It should be noticed that, in the engine used, the diagram was nearly parallel, so that it did not bring out to anything like the full extent the errors which would exist with a highly expansive engine. Although they made some attempts to carry out experiments with expansive engines, they found it impossible to do so for want of the necessary facilities. Nothing could be done without a brake, which did not exist on many engines.

Another point of great importance was what might be called the effect of the stretching of the string in moving the card. He did not think that that effect had been suspected as producing any important result on the indicator, nor did he himself at all suspect it before the investigation was begun. The first defect noticed was that on substituting one cord for another, a very different length in the diagrams was given by the two cords, a difference of 10, 15, or 20 per cent. There was also a discrepancy which they could not at the time account for. Every one who had examined a diagram had been puzzled to know how it was that all the phenomena which must occur on the admission of steam were entirely obliterated, and the whole crowded up into a straight line at the admission end. When they came to examine the matter, and work it out, it appeared that the diagram was distorted, that the friction on the drum, arising partly from the pressure of the pencil, acting always against the motion of the drum, would act to increase the length of the string in one direction, and to diminish it in another. That was worked out very carefully, as Mr. Brightmore had shown, by pricking holes in the diagram actually showing the effect.

When they first discovered this effect, they were using a new cord on the indicator, and they got a larger effect than appeared in the diagrams. But the amount of stretching, the obliquity of the dotted line proved in the diagram would, with a large degree of expansion, be sufficient, as he had proved in the Paper, to affect the area of the diagram, Fig. 8, to the extent shown between the continuous and the dotted lines, the dotted line showing the restored diagram and the continuous line the actual diagram. The increase of length would be in the thick part of the diagram before the cut-off, and the result would be that the mean pressure in the diagram would be increased something like 8 or 9 per cent. The fact that that effect did take place was evident from the common form of diagrams, which gave the admission. There was a straight line showing admission, and a sharp point at the top.

Professor Reynolds. Diagram Fig. 10, with string, was taken from the same engine as diagram Fig. 9, with wire. In the one case there was a vertical line, in the other a more or less inclined line rounded at the top, and it came down with a fair oscillation. Fig. 10 was a common form. It proved that the diagram hàd been distorted, that the piston moving regularly the indicator-drum stopped when it reached the end, and remained unmoved till the string was slackened and began to stretch again; consequently the indicator-drum did not start till after the piston had got a fair length of stroke, and in the meantime admission had gone on in a straight line. He could not say that they had made an experiment with a high-expanding engine to prove the fact: that would remain for others to do. They had proved the distortion in the case of an ordinary indicator, one of the best that could be obtained. They had demonstrated that with the stretching shown in diagram, Fig. 8, that amount of difference was obtained. Taking another diagram from Richards indicator, and restoring it by the same rule, they obtained the dotted lines, diagram Fig. 11, instead of the full lines. It would be seen that the cut-off on one side of the engine was much less than the cut-off on the other side. The length of the diagram on one side of the engine was considerably less than that on the other, about the same distance as the cut-off. That was a common feature in indicator-diagrams. In restoring the diagrams account had to be taken of the length of cord used in the front and in the back diagrams, the latter being nearly twice that of the former, and the result was to restore the cut-off, and to bring the expansion shown in the diagram into accordance with the true hyperbolic expansion-line for cut-off. The proof of this was that while in the diagram as published the expansion-line of the steam was as shown, in the other case the upper continuous-line was exactly the same shape. That explained a good deal which could not otherwise be explained, and it seemed very strongly to imply that many of the diagrams of expansion given were much too small. For the investigations they had undertaken they had no animus against any indicator; their object had been to improve the method, and not to diminish faith in the indicator itself. He had made many experiments with other indicators, but he chose that in which he thought the greatest confidence was placed. He believed that whatever other indicator might be used it would show similar effects.

Mr. Marten. Mr. E. B. Marten said the present lull in business was a good opportunity for engineers to look to their weapons, and sharpen their tools for future work. In the two Papers under

Mr. Marten.

discussion there had been admirable hints for better use of the indicator. They had used it for many years, and had been obliged to trust in it with a very indefinite notion as to how far it was wrong; and the Institution was much indebted to Professor Reynolds foi having brought forward the matter so clearly. He had always had a great desire to ascertain from numerous indications of engines what was the work commonly done—not by experiments in which everything was of the very best that could be made — but by engines working under ordinary circumstances. Some people had very little faith in the indicator as generally used, besides they did not want to have their engines compared in ordinary practice with sample experiments, lest they should appear to a disadvantage. Both the Papers would, he thought, tend to diminish the dread of the indicator in future. He should like to ask why it was necessary to take the indication of the speed of the engine by a somewhat complicated apparatus. The meeting had been greatly interested in the excellent comparison between the absolute speed of an engine, taken by electricity, and the indicator taken in the ordinary way, showing the stretching of the cord. He had been encouraged to find that a good deal that had been said against the indicator was not necessarily against the instrument itself, but had reference to the mode of applying it. Nothing had been said as to the frequent difficulty of applying the indicator where it could not be put very close to the engine; or as to the errors that had arisen from having it a good way from the engine. It was often put on pipes very neatly arranged, and where both ends of the cylinders could be on the indicator alternately; but this led to serious error, which was often altogether overlooked. The closer the indicator was to the cylinder the truer tale it would tell as to what was going on within.

Professor Reynolds.

Professor Reynolds said Mr. Brightmore had mentioned the throttling of steam in the indicator. It might, perhaps, be convenient to explain the two dots going through the paper. The apparatus consisted of the slide of the engine moving backward and forward with the cylinder, making a contact at the two ends at nine intermediate points, so as to give ten equal spaces. It went backward and forward at exactly the same points, so that if the diagram were true, the electric current would pass through the diagram at points vertically over one another. The holes shown were pricked by the current in the diagram. In the wire they were nearly vertically over one another, as they should be. The two points occurred when the piston was in the same position.

Professor Reynolds. If no diagram was taken, but the engine merely made to run backward and forward on the atmospheric-line, the holes were pricked nearly simultaneously. When they came to use the string on the indicator there was an entire absence of verticality. The indicator was much behind its position when moving forward, and again behind its position when moving backward. The want of verticality in those points showed how far the diagram was distorted by the stretching of the string which worked the drum.

Mr. Rich. Mr. W. E. Rich thought the Papers had dealt admirably with the errors, which could be examined by mathematical analyses and mechanical experiments, such as had been described; but in applying the instrument in practice precautions must be taken in many other ways. It must be perfectly clean and warm. He had seen Sir Frederick Bramwell lag an indicator when it was exposed to the breezes on an open-decked high-speed launch. It must also be suitably lubricated. A great many errors crept in from too much friction in the piston of the indicator. It was not every lubricant that was suitable. In a steam-engine using high-pressure steam a lubricant that would decompose under high temperatures was inapplicable, and the experiments would be comparatively valueless, owing to the clogging and friction due to the presence of the particles of decomposed unguent in the indicator-cylinder. Then it was desirable that the engine should give dry steam, because wet steam would produce considerable friction, and upset the results. Priming was fatal as long as it lasted, and indeed long afterwards, if dirty water was primed over. He had found long indicator-pipes most prejudicial to accurate results. In 1869 he worked for a long day in a flour-mill, testing the indicated-power taken by every pair of stones, and every bushel of corn ground. On returning home and examining the diagrams he found that they were unsatisfactory in many respects, and were much rounded in the corners. There were two Woolf beam-engines with long indicator-pipes, from the top to the bottom of each cylinder. He had one indicator for each cylinder, and with the four cylinders he had sufficient to do; but on inspection he could not trust the diagrams. On the following day, therefore, he went over the experiments again, with all the pipes disconnected and the indicators close to the cylinder ends, and he then obtained much larger diagrams. It thus became evident that many of the powers deduced from the first day's experiments were below the truth, the errors in some being as much as 15 per cent., so far as he remembered, in consequence of the length of the pipes alone. He did not know all the causes at work, Mr. Rich.

but among them were chilled gases, wet steam, and water, and the inertia of getting such mixtures into motion, and all those elements were prejudicial to the obtaining of accurate results. Then, in high-speed engines, the vibration of the instrument itself was sometimes very prejudicial. In indicating a ploughing-engine in the field, making from 300 to 330 revolutions per minute, the whole engine shook to such an extent as to affect the diagrams appreciably. There was also another thing which a beginner had to guard against, and he was afraid that sometimes experienced persons neglected it. The string should be so adjusted as to allow the paper-cylinder to vibrate between limits perfectly clear of the end stops. The higher the speed the greater the latitude ought to be at the ends. The Author had referred to the Royal Agricultural Society's trials at Cardiff in 1872, and to the use of the friction-brake as being one of the checks upon the indicator; but of the twelve engines tried at Cardiff four at least gave indicated results which could not be trusted considering the powers developed at the same time on the brake. Three of those gave indicated powers only 2½, 6½, and 9 per cent. respectively more than the brake-powers, results which it was known must be impossible, as they were by no means the best engines there, and the fourth was the first-prize engine of the trials; and that engine gave 2 to 5 per cent. more brake-power than indicated-power, although every kind of spring was tried, and the indicator was cleaned and changed several times. One of the reasons was, that there were very small apertures through the indicator cocks in the last-named engine. He believed that many marine-engine trials, even in the Royal Navy fifteen or twenty years ago, were extremely erroneous, the indicated powers at that time being often much larger than they should be. This would be seen on comparing the results recorded for some ships tried with the old McNaught indicator fifteen years ago, with others obtained a few years later with the Richards indicator, as they showed a difference of 20 per cent. in the resistance-coefficients of similar ships. No doubt the old springs were kept well polished and were rarely tested, and consequently, as they grew older, brighter, and lighter, the reputation of the marine engineer of the day improved, while that of the naval architect waned. In taking indicator-diagrams, there were many practical precautions to be taken beyond those which he had mentioned. One of them had reference to the governor. If it was a very delicate one, or at all liable to "hunt," it was difficult to get accurate indicated results. He was sure that many results of engine-trials were often quoted, founded on

Mr. Rich. indicator-diagrams which, if they were thoroughly investigated, should not be trusted within 10 or sometimes even 20 per cent. The indicator was, in his opinion, an invaluable instrument for recording the behaviour of steam in the cylinder, and the action of pump-valves, and also as a power-meter; but great experience and care were necessary in the use of it when accurate results were to be obtained.

Professor Cotterill. Professor J. H. Cotterill said he had no observation to make on most of the different causes of error to which Professor Reynolds had alluded, such as the effect of temperature on the springs, the oscillations from the springs not being strong enough, and the inertia of the parts of the indicator. Every engineer of experience had no doubt had his attention called to those matters. Engineers were also, no doubt, familiar with the effect of the inertia of the drum in unequally stretching the cord. The use of wire instead of cord had been recommended at least ten years ago in a well-known treatise on the indicator. It had also been stated that it was impossible to take an accurate diagram when the cord was too long. Professor Reynolds had, however, done good service by pointing out a special effect of the friction of the drum, which had been distinctly perceived by few, if any, writers on the subject. This effect was, he imagined, very difficult to detect, from the circumstance that it was independent of the speed, and probably might have occasioned errors of some magnitude even in diagrams taken by practised hands. The formula given in the first Paper was interesting, and, if the value of the constant was adopted, showed that where the cut-off was early the error might be very great. With a cut-off of one-fourth, however, the error was only 5 per cent. even with a cord 8 feet long, and besides, it might be conjectured that the Author's value of the constant was much too large to apply to the best practice. The fact that, when corrected by Professor Reynolds, certain diagrams showed an expansion-curve differing little from what he called the "true" curve, proved that the correction was too great. For by the "true" curve the adiabatic curve was evidently meant, and consequently, as was further shown by sundry other expressions in the Paper, the action of the sides of the cylinder was assumed to be negligible. In fact Professor Reynolds had completely ignored a mass of experimental work, much of which might be compared, in conscientious care and insight into possible causes of error, with similar work done in a physical laboratory. Eight years ago, in writing on the theory of the steam-engine, Professor Cotterill had stated that the great magnitude of the action of the sides was conclusively proved.[1] This opinion was formed after a careful study of the well-known experiments by Mr. Hirn. In this writer's works much reasoning might be found which few physicists would be prepared to accept, but this did not affect the truth of his experimental work, which assuredly was free from the gross errors insisted on in the Paper. Since then much had been done, of which the valuable experiments by Mr. Mair need only be mentioned.[2] All such experiments substantially agreed, and many had been checked and counterchecked in various ways.

Professor Cotterill.

Besides this, an important part of the evidence was independent of the accuracy of the indicator employed. When an engine was tried first with and then without a steam-jacket, all other conditions remaining the same, it had been found that a small amount of heat supplied by the jacket would produce a large addition to the indicated work—a fact which alone conclusively proved that the sides of the cylinder stored and restored large quantities of heat. So again, two experiments at different steam-pressures, the cut-off and all the other conditions remaining the same, would show that the higher pressure gave the greater economy.

In making these remarks, however, he was not disposed to deny that a sensible part of the enormous initial condensation when the cut-off was very early might possibly only be apparent, on account of the effect of drum-friction so clearly pointed out by Professor Reynolds. This condensation had been supposed to amount to 50 or 60 per cent., most part of the condensed water being re-evaporated during expansion. Unless some special cause of accumulation of water existed, a condensation of more than 25 to 30 per cent. involved considerable difficulties, and should only be admitted on the most unimpeachable evidence. In conclusion, though he believed that the reliance placed on indicator-diagrams by a practised experimentalist was the result not of "confidence," but of a thorough practical knowledge of the capabilities of the instrument and the errors to which it was liable, yet he fully agreed that a systematic investigation of the kind undertaken would lead to results of considerable value.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, President, said that perhaps Mr. Marten would be pleased to hear a very striking instance of the errors that might arise in a diagram from a too great length of pipe. Many years ago, when he was little more than a lad, he had the privilege of being present at the indicating of an oscillating-

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[1] "The Steam-Engine considered as a Heat-Engine," p. 216.

[2] Minutes of Proceedings Inst. C.E., vol. lxxix., p. 323.

Sir Frederick Bramwell. engine driving a screw-propeller. In those days the ability which afterwards prevailed in the taking of indicator-diagrams from a rapidly oscillating cylinder had not been attained, and there was a great deal of scheming as to how the diagram should be obtained. An ingenious gentleman who had charge of the experiment thought that a flexible pipe offered the best means for the purpose, and accordingly such a pipe was placed between the top of the cylinder and the nearest fixed point in the engine, to which the indicator was attached. The indicator-diagram was obtained, but its ordinates were altogether out of time with the stroke of the piston, showing how the indicator-diagram might be seriously affected by the too great length of the pipe. Another point—In 1854 he went to Marseilles to indicate the combined ether-engines of the Du Trembley[1] steam-boats. The ether-pressure was not great, and as probably most of those present knew, the heat of its vapour was extremely small, and the diagrams first obtained were meagre. Feeling sure from the performance of the engines that the ether-cylinder must be doing more work than was to be gathered from the diagram, and thinking it possible that the ether vapour was being rapidly condensed in the indicator and the pipes leading thereto, he swathed the whole in flannel, and then obtained a diagram showing a fair amount of pressure in the ether-cylinder. Then came the difficulty of the indicator suddenly becoming set fast. It appeared that the ether had acted upon the oil, and made a gum inside the indicator, causing the piston to stick. This was overcome by using a glycerine lubricant, which seemed to answer very well.

Mr. Mair. Mr. J. G. MAIR remarked that, unfortunately, too much confidence was put in an instrument which engineers were daily in the habit of using (and the same remark applied also to theodolites, levels, transit-instruments, &c.); it ought to be the duty of every engineer or scientist to realize the fact that when an instrument was being used, it should be treated in a proper manner, and the readings taken with care, so as to avoid such errors as were incidental to that special instrument. But he thought that the Authors were rather hard upon engineers who used the indicator, because they implied that they had confidence in the instrument on the ground of the apparent simplicity of its action. Those members, whose special duties in the profession did not lead them into daily contact with the indicator, were liable to infer that no care had been taken to

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[1] Minutes of Proceedings Inst. C.E., vol. xviii., p. 233.

Mr. Mair.

thoroughly test the instrument. The subject, however, had been investigated many years ago. Professor Berndt, for instance, in 1874, had thoroughly gone into the question of indicator-springs; he tested them when hot and when cold, and found a difference amounting to about 2½ or 3 per cent. Mr. Porter, also, in his book on the indicator, had stated that the error of indicator-springs, as between cold and hot, was 2½ per cent. Every engineer who knew his business would test the indicator-spring when he received it, however reliable the maker might be. The best plan for those who were in the habit of using rough and ready methods, was to tack an indicator on to a mercurial pressure-gauge, to pump up the mercury with a hand-pump, and force it to a given height. By doing that with a 30-lb. spring, instead of getting the $\frac{1}{30}$, or rather $\frac{1}{120}$ inch, sixty times the motion of the pencil would be obtained, because 2 inches of mercury were equal to 1 lb. pressure. That was by far the best way of testing indicator-springs. At Grosvenor Road, where many trials had been made with the indicator, he had always tested the instruments on that principle. It was better than using the cathetometer, because the area of the piston need not be measured. The effect of the inertia of the piston and the oscillations of the spring had been examined by Mr. L. de Maupeou in France.[1] He had studied the subject, and had given formulas for the oscillations, which were practically the same as those enunciated by Professor Osborne Reynolds. In pumping the mercury against the indicator, at a certain height the pencil of the indicator was drawn on the indicator-barrel where the paper was. In coming down it behaved differently; it lagged behind, and that was a point the cause of which he had not been able thoroughly to ascertain. He should be glad if Professor Reynolds would give the reason why the indicator lagged behind the mercury-pressure when coming down. Professor Reynolds had stated in his Paper that the friction and the inertia of the drum-barrel up to the present day had been entirely unsuspected. No doubt it had been unsuspected by Professor Reynolds, to whom great credit was due for his evidently original investigations. Professor Berndt[2] had, however, investigated the question of the stretching of the indicator-cord, for which purpose he had used an ingenious instrument devised by Professor Weinhold, and marked the paper electrically as was done by Mr. Brightmore, and he had found

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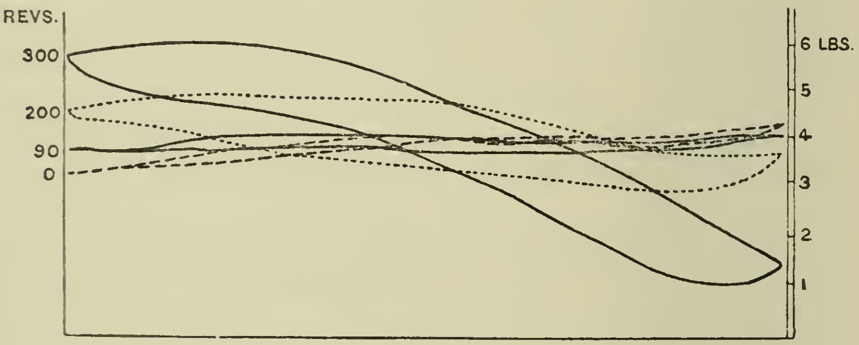
[1] Minutes of Proceedings Inst. C.E., vol. lxxii., p. 386.

[2] "Engineering," vol. xxv., pp. 77 and 295.

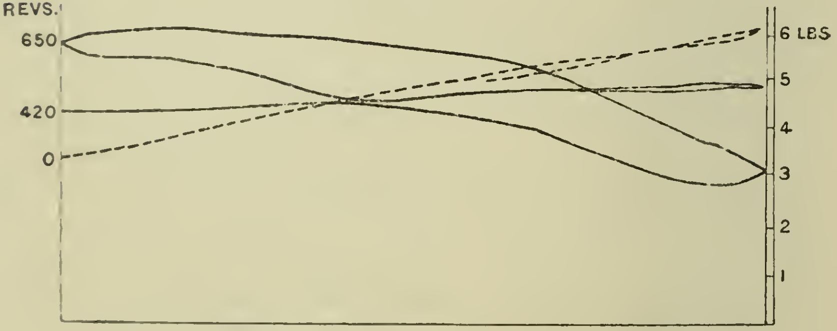
Mr. Mair. that the errors due to it were not more than ½ per cent., which was no more than might be fairly expected to arise from errors of observation. If there was a uniform strain on the indicator-cord, and there was no lost pressure at the end of the stroke, but the same as there was at the commencement, the indicator would of course move isochronous with the piston. In America the question had been fully studied, and an instrument had been devised for ascertaining the tension on the cord, for when that was ascertained the relative amount that the indicator would lag behind the piston was known exactly. He had placed on the table an apparatus of American construction, called a Brown's device,[1] which was used to ascertain the strain on the cords of various indicators at different speeds. The diagrams showed the varying strains on the cords for four indicators, viz.: "Richards," Fig. 35; "Crosby," Fig. 36; "Tabor," Fig. 37; and "Thompson," as made by Messrs. Schäffer and Budenberg, Fig. 38. It would

Fig. 35.



Richards.

Fig. 36.



Crosby.

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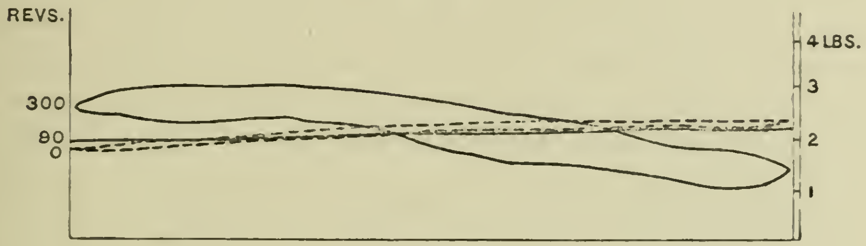
[1] Fig. 51, p. 77.

Mr. Mair.

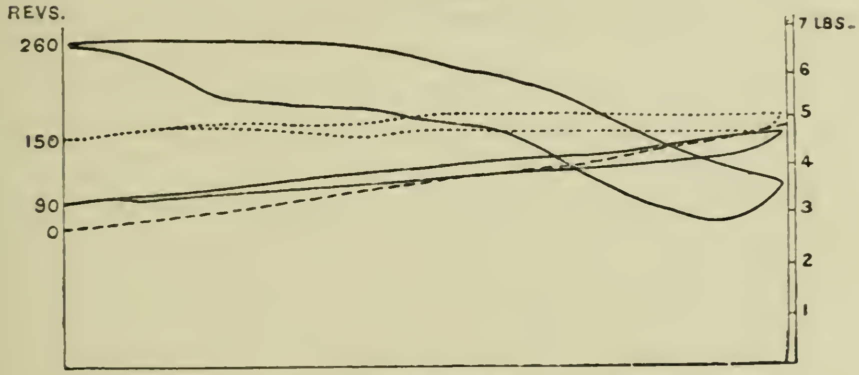
be seen from Fig. 39 how the different forces acted upon the cord; of course the line showing the inertia was not a straight line, but that was sufficient for his illustrations. On the diagrams (Figs. 35 to 38) a dotted line was shown, which was made when the

FIG. 37.



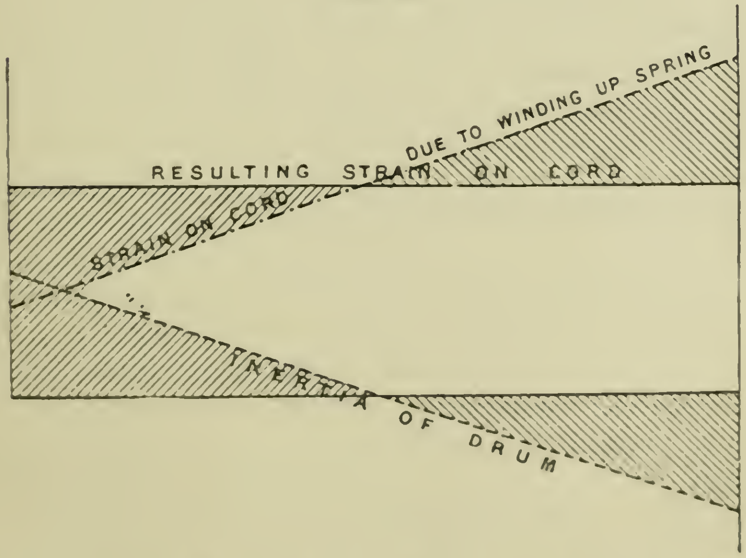
TABOR.

FIG. 38.



THOMPSON.

FIG. 39.



Mr. Mair. indicator was moved very slowly, say 1 revolution per minute. The ordinates represented pressure, and the abscissas the length of travel. It would be noticed, that in the Richards indicator (the one tested by Professor Reynolds) the tension on the cord would vary from about 5 lbs. to $\frac{3}{4}$ lb. at 230 revolutions. If the pressure on the cord varied to that extent, and the cord was elastic, the position of the drum would shift in comparison with the motion of the travel of the piston. But a Richards indicator was not a suitable instrument to run 230 revolutions a minute, as would be evident from the diagram. To make the diagram a perfect one—to have an indicator that would run isochronous with the piston—there ought to be a drum-spring that would come much higher than the spring used; in fact, a spring was wanted with greatly increasing tension. It was evident also, that the Thompson indicator was not a good one for 230 revolutions; it was limited to about 150. The difference between the forward and the backward stroke gave an alteration of tension in the cord, but it was so small as practically to make no difference in the length of the cord. The best indicator for high speeds was the Crosby. It had a very light drum, the reciprocating parts being exceedingly light, and the inertia consequently was small; and the tension of the spring could be increased to vary with the speed. With a different spring 650 revolutions might have been attained; but not having the best spring the diagram was not so good. At 420 there was a very fair diagram indeed, and consequently a very uniform tension on the cord, showing that the Crosby indicator was a much better one to work with than the Richards at a high speed. The trials made by Professor Reynolds were principally with an indicator that was not used for high speeds, but always for low speeds. A perfect indicator should have no mass or weight in its reciprocating parts; but as that could not be attained, the indicator must be made as light as possible, namely, the barrel-motion and the pencil-motion; at the same time the drum-spring should be in proportion to the motion or the inertia of the reciprocating parts, so that a straight line might be obtained on the diagram. Professor Reynolds had said that if oscillations crept into an indicator-diagram, the pencil was pressed against the paper to take them out. It was hard upon engineers that such a thing should be said. They did no such thing; they took the spring out of the indicator and put a stronger one in, or shortened the stroke of the indicator. He should not like it to go abroad to Germany and America, where great attention was paid to the question, that the indicator was used in that way in England. In conclusion, he wished to draw attention to the opening statement in Professor Reynolds' Paper, with regard to Mr. Hirn, from which it would appear that Professor Reynolds thought that Mr. Hirn did not believe in indicators. The statement was made some twenty-nine years ago, before the Richards indicator was invented, and since that time Mr. Hirn had written, that in spite of the unfavourable impression he formerly had, he was convinced that the indicator diagrams taken by Mr. Leloutre were within $\frac{1}{200}$ of the truth.[1] Mr. Hirn also tried an indicator against the readings given by a flexion dynamometer, attached to the beam of an engine, and found them to agree closely. So convinced was Mr. Hirn of the correctness of the diagram given by an indicator that he measured the diagram, and absolutely relied on it for temperatures and volumes of steam. Mr. Mair.

Mr. E. A. Cowper thought it was advantageous to have a discussion raised upon an old instrument, but it was much more advantageous if it was raised upon a new instrument, so superior to the old one as entirely to displace it. On the present occasion the Authors had evidently not thoroughly investigated the practical use and application of the best indicators. Much depended on the manner in which the indicator was used; for instance, a friend of his once made a pair of engines for an American. An Englishman, who had no experience in indicator-figures, took the figures off the engine, and such figures he believed had never before been seen. He connected the string to the eccentric rod, which had a totally different time from the piston-rod, and the consequence was that a figure something like the letter Z was made. A second engineer who had been consulted then took the figures, which proved the engines to be right. It was more than forty years ago since Mr. Cowper became intimate with the indicator. Having taken the figures of a number of different machines, he found that he had a very close approximation to the exact total for all the machines. A friend said, "You are quite wrong. There are six or seven machines all alike, all going at the same speed, but you make one of them to take 50 per cent. more power than the others." But in a few days, he had the pleasure of receiving a note, saying, "You are perfectly right; the man working at that grinding-machine supplies the grinding material and the water better than the other men, and he does 50 per cent. more work. In future I shall make all the men do the same work." This was Mr. Cowper.

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[1] "Théorie Mécanique de la Chaleur," 3rd edition, vol. i., p. 68.

Mr. Cowper. an example of useful work done by the indicator. As the Authors had taken the trouble not only to commence a few simple experiments, but to give the results to the Institution, it was a pity that they had not a good engine with which to experiment. Engineers, now-a-days, seldom had to do with such a poor specimen of a "coal-eater," for it had not even a reasonable pressure of steam; it had no vacuum, and no variable expansion, and, indeed, practically no expansion, so that very many actions and effects could not be obtained at all from such an engine. He doubted if it had a steam-jacket. It was to be hoped that Owens College would soon possess a better engine. The beauty of an indicator in action was when it followed well a long expansion-curve, giving the exact point of cut-off, the precise vacuum, and the exact commencement and extent of the compression. He hardly liked to enter much into the details of the matter, which were well known and appreciated by engineers. Those who had taken hundreds of figures from different engines knew well that the passage for steam must not be throttled, and that a different figure would be obtained if it were throttled. If the piston and the paper-cylinder spindle were not oiled the figure was spoilt. If a new and stiff cord was used the figure would be untrue. A new cord could always be brought into the same condition as an old one, in about five minutes, by pulling it round a pin, and stretching and smoothing it down. The result would then be true to within 0·5 per cent. If the distance was great, it was best to use a light rod instead of a long string. If a wire was used, it would sag from its own weight, and although it might be a little better than a bad string, it was not better than a good one. He might be permitted to direct attention to the way in which the power of an engine was stated. After the indication of its power of working had been obtained, it was called "indicated horse-power," not "actual horse-power." The instrument gave an indication of what was going on in the engine, and it gave a near approach to the actual horse-power. He denied that the instrument deceived those who knew it well; no doubt it had its defects as well as its virtues. It had been carefully studied and described by many, and amongst the books that had been written about it since 1824 might be named those at foot.[1] If the spring was not weighted when hot, the figure

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[1] "A Treatise on the Richards Steam-engine Indicator," C. T. Porter, 1874. "Elementary Treatise on Steam and the Use of the Indicator," Graham. "The Indicator Diagram practically considered," N. P. Burgh, 1869. "The Working of the Steam-engine explained by the Use of the Indicator," J. Hopkinson and Co., 1875. "Mechanics' Oracle," Plate, July 1824. "Description and Use of the

Mr. Cowper.

was a certain percentage too high, though it would still show the action of the valves as well as if it had been weighted when hot. If the pencil was pressed too hard on the paper there would be an imperfect figure, and if the pencil was not properly rounded and smooth-pointed the same effect would be produced. If the paper cylinder was too heavy, it ought to have a strong spring to make it follow well. If the piston of the indicator was too heavy and the engine worked quickly, an error was introduced. It was a mistake to suppose that the area of the figure was wrong because there were undulations in it. He had many years ago carefully taken out the exact area of such a figure, and compared it with others with the same indicator without undulations, and the areas were precisely the same. In fact, it stood to reason that the pencil, fluctuating above and below the true line and being quite free, ought to describe the same area. It was as though a string were stretched tight and then vibrated; its average position would undoubtedly be the same as though it were not vibrated. If it was desired to get the true line as well as the line drawn with undulations, a skilful operator would put his thumb for the piston-rod to bounce against, then the piston-rod would strike his thumb, and give a smooth line without undulations. The friction in the indicator-piston and pencil was balanced, and often more than balanced, by the fact that they had some momentum, even if made light. Therefore the pencil always reached the steam-line. Then when the paper was moving horizontally, the friction of the pencil vertically was almost annihilated, and the pencil followed the pressure admirably. There were many other points well known to practical engineers. In Watt's patent of 1782, a good indicator-figure was shown cutting off at one-fourth. That was not taken, he believed, by an indicator, because an indicator was not mentioned in the patent, although the indicator-figure was shown; but at the South Kensington Museum there was a cylinder and piston of Watt's with a spiral spring to resist the pressure, and he had no doubt that that was the origin of the indicator.[1] It was a cylinder connected with the cylinder of the engine, with a quadrant at the top, a spring on the other side to resist, and a

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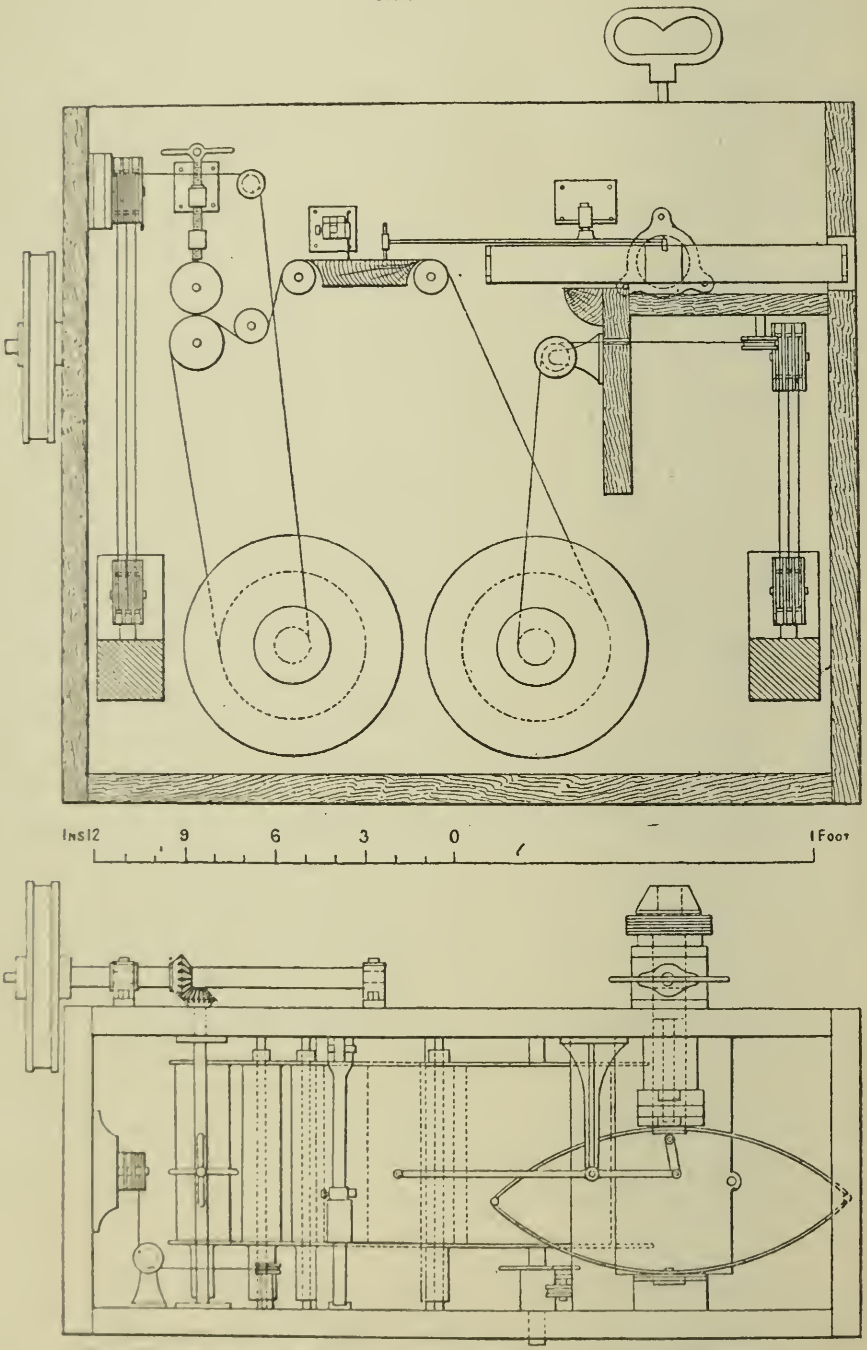
Indicator," Chadburn, 1838. "A Treatise on the Steam-engine," chap. vi., p. 48, J. Farey, 1827. "Encyclopædia Britannica," 3rd edition. "The Indicator and Dynamometer, with their Practical Applications to the Steam-engine," T. J. Main and T. Brown, 1854. Engineering, vols. xxiv. and xxv. "Sur les Indicateurs," Mosseau. Transactions of the American Society of Mechanical Engineers, 1883, Barrus. "Guide pour l'essai des Machines à vapeur." Par J. Buchetti, 1884.

[1] Institution of Mechanical Engineers. Proceedings 1883, Plate 64.

Mr. Cowper. hand to indicate the pressure. It did not carry a pencil, but it showed the degree of pressure.

Figs. 40.



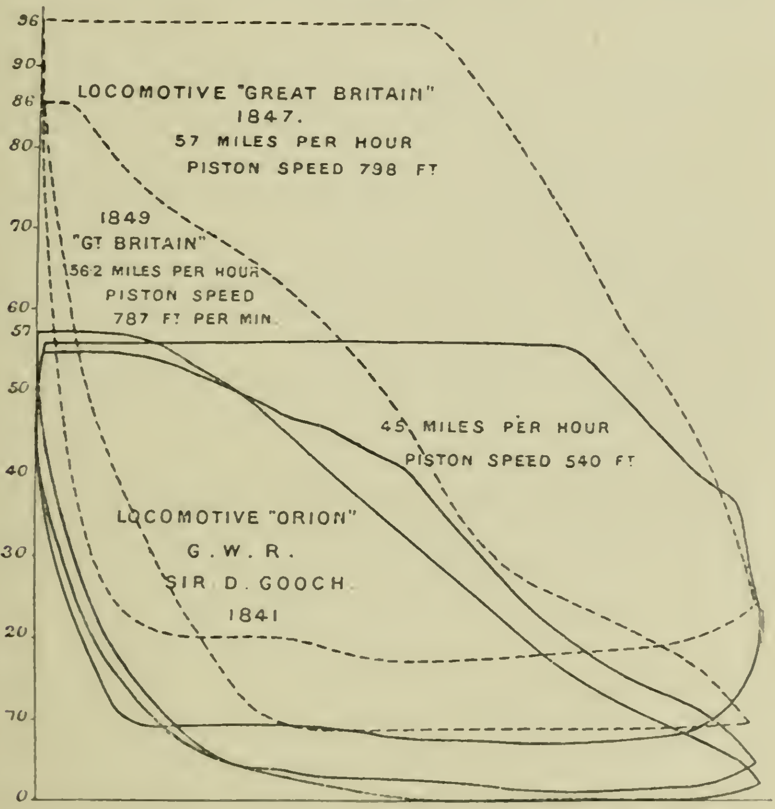
Sir Daniel Gooch's Indicator for Locomotive Engines.
½ full size.

He exhibited a few indicators of somewhat different constructions. The first was Watt's indicator, with a sliding-board for the

Mr. Cowper.

paper. A second indicator was a small one by Chadburn, which went up to 20 lbs., and a large one going up to 100 lbs. One indicator had been proposed with a paper barrel outside the cylinder, but the friction was too great, and it was thrown aside. Moseley's indicator multiplied the pressure and motion, and gave the results in figures. A fourth indicator was the invention of Sir Daniel Gooch, who had kindly allowed it to be brought from Swindon to be shown to the members. It was made in 1840 for the express purpose of taking indicator-figures at high speeds on locomotives.

Figs. 41.



It worked well up to 60 miles an hour. The result was obtained by a large cylinder—viz., 1 square inch area—and the piston working a long lever, the pencil moving through a considerable arc, and the paper moving continuously from one barrel to another. The paper was a long sheet, and unrolled from one barrel over the table where the pencil worked on it, down again on to the other barrel. Figs. 40 were a plan and sectional elevation of the instrument. The actual indicator-figure could be easily made from the continuous diagram drawn by the pencil.

Mr. Cowper. Figs. 41 showed three indicator-diagrams (in full lines), taken from the locomotive "Orion" at a speed of 45 miles per hour, the cylinders being 15 inches in diameter by 18 inches length of stroke. The wheels were 7 feet in diameter, the piston-speed 540 feet per minute. The two diagrams in dotted lines were from the "Great Britain," at speeds of 56·2 and 57 miles per hour, the cylinders being 18 inches in diameter by 24 inches length of stroke, with wheels 7 feet in diameter. The piston-speeds were respectively 787 feet and 798 feet per minute, at which speeds there was no difficulty in obtaining good figures with Mr. Gooch's indicator. In 1858 Mr. Cowper designed an instrument for stationary engines, much of the same kind, except that the area of the piston was 3 square inches. A fifth indicator was one of Richards' common indicators with ½ square inch area of piston, and going up to 175 lbs. per square inch. In conclusion, he would say that so much reliance could practically be placed on the instrument, that he would recommend any one wishing to make a first-class engine, to use it freely on both ends of both cylinders, on the "hot-pot," on the steam-jackets, on the condenser, on the air-pump, and on the boiler, as it was very desirable to know what was going on inside the engine. He did not think the Author need fear that any thing he had said would have the effect of diminishing reliance on the indicator itself, as it was too well known.

Mr. Harris. Mr. H. G. Harris desired to join his protest to that of the previous speakers, against the statement of Professor Reynolds, that engineers had for years past put confidence in an instrument which was sometimes as much as 15 per cent. wrong. Surely if the instrument were so erroneous, engineers must have long since recognized it, and must have endeavoured to cure the defects. Of course if the instrument was not properly treated errors would arise. Mr. Mair had mentioned the endeavours which had been made in the Crosby indicator, to cure the liability to error. In early days when pressures were low, and speeds were low, the Watt indicator was sufficiently accurate for the purpose for which it was devised. Then when speeds and pressures increased, the Richards indicator and various others to which Mr. Cowper had directed attention were introduced, and now when speeds and pressures were higher, an instrument was required superior even to the Richards indicator. Manufacturers had endeavoured, and he believed successfully, to provide such an instrument, and in doing so had cured the defects of which Professor Reynolds had spoken.

Mr. Harris.

It was suggested by Mr. Brightmore's experiments that all indicator-springs were erroneous in their records when hot, and that sometimes the error was as much as 2½ per cent. It appeared strange that manufacturers should for years supply springs which were liable in ordinary use to an error of 2½ per cent., and he had endeavoured to ascertain whether this was so or not; but he found that the makers of the indicator which had been so frequently referred to (the Crosby indicator), professed to rate and grade all their springs at a temperature of 212° Fahrenheit, and further, the makers of the Richards indicator assured him that the error they found by frequent experiment to exist in the cold springs was allowed for, and the springs made by them were correct when hot.

It had been stated by Mr. Mair that the drum-spring should have an increasing tension as the drum was unwound, and that it should not be, as Professor Reynolds had said, a spring which would give as nearly as possible a constant tension as it was unwound. He agreed with this statement, and in the reasons Mr. Mair had given for it, which appeared conclusive.

It had been pointed out by Mr. Cowper how many things the indicator showed when it was properly treated. It demonstrated, with sufficient accuracy, what was going on in the cylinder. When it was put upon the steam-jacket it showed also whether the steam-pipes were too small. In fact, so useful was it to the engineer, and so accurate were its records, that he believed the majority of the improvements in steam and other similar motor-engines, which had been made during the last few years, were due almost entirely to the facility of discovering by the indicator what was going on in the cylinder.

Professor Kennedy.

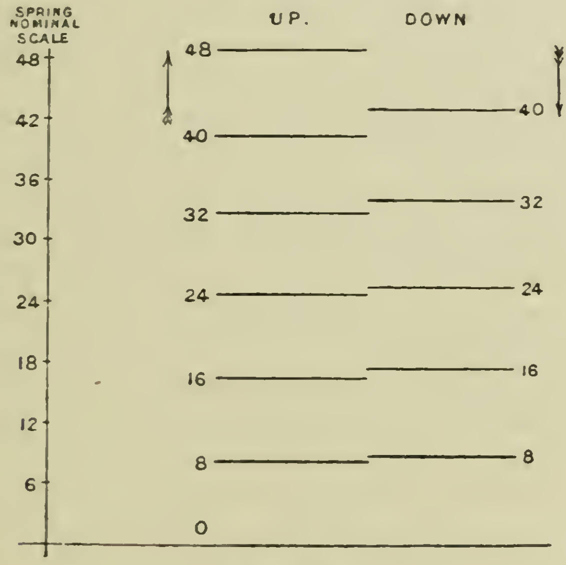
Professor ALEXANDER B. W. KENNEDY believed it was rather extreme affection for the indicator, and not any grudge against it, that made many engineers anxious that it should be right. Indeed, he believed that, after all, Professor Reynolds and others were truer friends to the indicator than the last speaker. When engineers saw, as he thought they did, in the *Times* every now and then, that the indicated HP. of one of her Majesty's ships was 5961·56, or when they saw steam used per HP. calculated at 17·6491 lbs. per hour, it would be admitted that some persons put an appalling amount of trust in it. If they were so to trust it, it certainly should be very carefully examined. In the interests of the indicator they ought to feel greatly indebted to the work that Professor Reynolds had done, and not to find fault with him for being too critical. The

Professor Kennedy. investigations which Professor Reynolds had mentioned in connection with the number of vibrations in the spring were new to him. He had been a great deal troubled with vibrations in the case of certain Richards indicators. A $\frac{1}{20}$ spring running at 100 revolutions gave too many vibrations to read the card comfortably. Calculating the quantity which Professor Reynolds called n, he made it about 22. A $\frac{1}{30}$ spring gave n about 26. In neither case was the card properly legible. Putting in $\frac{1}{48}$ springs he found that the card became quite clear; it was just over the limit mentioned, namely $n = 34$. With the Darke indicator, which he had used a great deal, he found that a $\frac{1}{20}$ spring gave a card without sensible vibrations at the same speed as before. He might mention that he had had four indicators at work on a little compound engine, with strings varying from 9 inches to 4 feet long. He had measured more than two hundred cards to ascertain the difference in the length of the cards, and he found that the mean length with a 9-inch string was 4·11 inches; with a 24-inch string 4·02 inches; with a 30-inch, 4·09 inches; and with a 48-inch string, 4·04 inches. They were excessively near together, and the differences did not always lie in the direction in which they ought to go if they were due to the stretch of the string. The cards remained constantly of the same length, and he had no doubt that he might have taken the average of five times as many without finding any sensible change (Table II, p. 67). The stretch of the string in Mr. Brightmore's experiments had been much greater than he had found to occur when the string used was of a proper quality, or else the results he had found must be considerably disguised by some other causes. He had recently conducted some experiments on indicator-springs, but most of his experiments had been made six or seven years ago. All the indicator-makers were supposed to test the springs when hot. The difference between springs when hot and when cold was considerable, but he did not think that engineers ought to use indicator-springs for serious trials without testing them, because although makers did their best, he had seldom found a spring which was by any means quite accurate. In that matter he differed from the Author, because he thought that the errors were much greater than they were asserted to be in the Paper. In the first place, there was a question whether the springs were uniform in their indications, that was, whether the same compression of the spring represented the same amount of pressure at all parts of the scale. He believed that generally this was the case. The second question was whether they were uniform at the par-

Professor Kennedy.

ticular scale marked on them; and he was bound to say that he thought they were often not so. Two springs supposed to be identical, being made by the same manufacturer, would occasionally differ 2 and 3 per cent. Like Mr. Mair, he had noticed a lagging behind of the indicator-spring as it came down. He had found it both with an apparatus something like that of Professor Reynolds, and with dead-weights hung on to the spring. The lagging behind was often very considerable (Table I, p. 66, and Fig. 42). With a $\frac{1}{45}$ spring, for instance, there was an average error, going upwards with increasing loads, of $-3 \cdot 2$ per cent., and coming downwards of $+2 \cdot 7$ per cent. If the quiet load were caused to vibrate, it diminished both those errors, but still left a considerable amount. After trying many experiments of this kind, he endeavoured to ascertain up to what height an indicator would give a line if he allowed a weight to come on it with reasonable quickness, keeping the pencil in contact with the paper, so that it might describe a line with some vibrations, finally becoming horizontal. In that case he often got a considerably better result than if he merely added or subtracted weights (Table I, "Admission-ordinates," p. 66, and Fig. 43). Next he made a great many experiments, which he thought were rather important. He loaded the indicator-spring with a considerable pressure, and he had the means of removing with tolerable quickness a portion of that pressure, so as to allow the pencil to fall. He allowed the

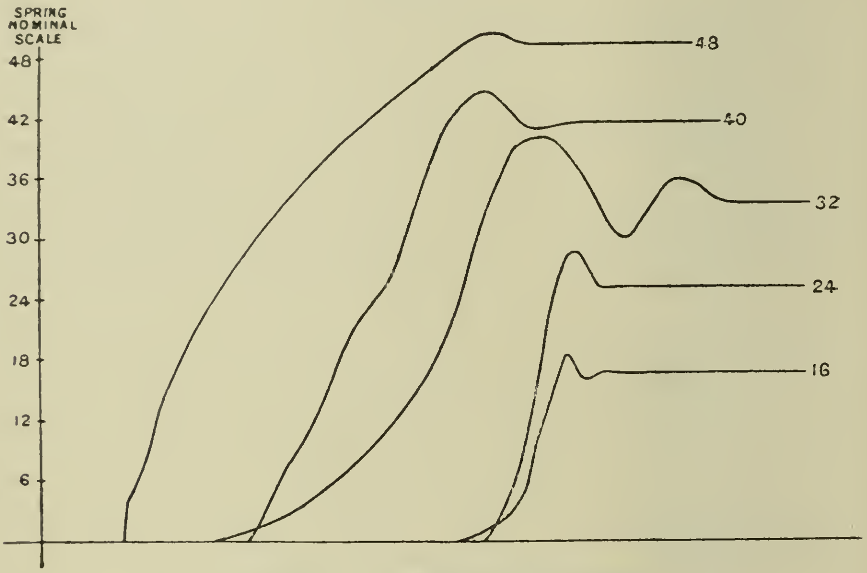
FIG. 42.



$\frac{1}{24}$ SPRING.

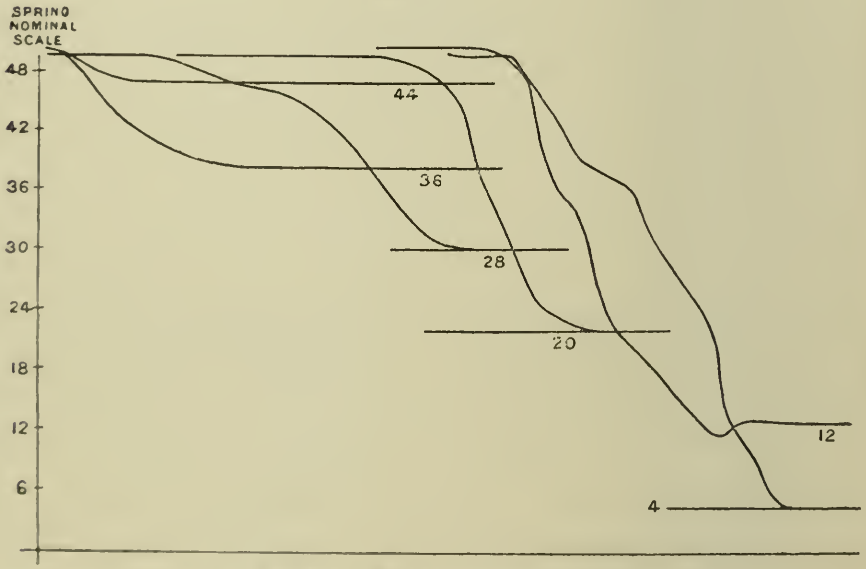
Professor Kennedy. pencil to trace the falling line until it became horizontal. In that case there was the pencil-friction, as well as other matters, against the pencil coming down; and he found in many cases that the

Fig. 43.



$\frac{1}{24}$ Spring. Admission-Ordinates.

Fig. 44.



$\frac{1}{24}$ Spring. Expansion-Ordinates.

ordinate was very much too great. In the spring he had mentioned, it was an average of 5·2 per cent. too high (Table I., "Expansion-ordinates," p. 66, and Fig. 44). He did not say that

Professor Kennedy.

the ordinates taken in that way exactly represented the falling of the indicator-pencil in an expansion-curve, but they were intended to imitate it as much as possible; and to such an extent as they did imitate it, it was shown that there might be a considerable error in the ordinates of the expansion-curve. He had also made experiments as to the differences when the pencil was in contact and not in contact, and those differences, with such small pencil-pressures as he used, were not large, averaging about 1 per cent. above or below the corresponding ordinates without pencil-pressure. He drew a series of horizontal lines separately, and then a series in which the pencil was caused to draw a vertical line between each. He made similar experiments with springs in tension, so as to test them as they might be tested *in vacuo*. There the errors were less, but of course the pressures were very small. Such figures as he was speaking of were the sum of the errors of the spring, with in some cases the piston-friction, and in all cases the parallel-motion. He had tried a number of different indicators, and had found that the difference between them was much less than between individual springs. He had had three springs, for instance, tested in two indicators. With a $\frac{1}{48}$ spring the error was 4·8 per cent. in one, and 3·8 in another. With $\frac{1}{30}$ springs the error was 7·8 per cent. in one, and 6·9 in another, and so on (Table III, p. 67). The result of his experiments was to show that the indicator-card was in general too large, and not too little; sometimes considerably too large. He had plotted out a number of indicator-cards as they ought to be, and as he believed the springs he had used would indicate them, and he had found them on an average 3 or 4 per cent. larger than they ought to be. He did not wish to lay too much stress on the absolute figures given, because his experiments were not quite under the same conditions as those under which the indicator actually worked. They were all made, however, with the intention of imitating those conditions as much as possible, and he feared that it was impossible to come to any conclusion except that the error in estimating power from indicator-cards drawn with untested springs might be very considerable, and would be almost always in excess. Fortunately, about the atmospheric-line the errors appeared to be generally least; and as one of the most important pressures that had to be measured was about that place, the result he had mentioned was a comforting one. He had not tested the Crosby indicator, though he had used it at high speeds. The lighter parallel-motion of the Darke indicator, even at the low speed of 100 revolutions or (200 feet per minute piston-speed), gave

Professor Kennedy. much better results than the somewhat heavier parallel-motion of Richards indicator. He added three Tables, containing the summarised results of some of his experiments. Table I gave the result of experiments on thirteen different springs, tested in all the different ways he had mentioned. Table II gave the results of the measurement of the length of cards; and Table III of some recent tests of the same springs in two different indicators, as to which point he would also refer to Note 1 to Table I. Figs. 43 and 44 had been prepared from tracings of some of the cards made during the experiments, and showed the "lagging behind" of the spring in

Table I.

Abstract of Results of Experiments on Indicator-Springs in Richards Indicators made by Alexander B. W. Kennedy.

Springs in all cases kept hot by steam of atmospheric pressure.

This Table is a summary of nearly 3,000 single observations.

Spring.	Percentages of Error.										
	Without Vibration.			With Vibration.			Admission Ordinates.	Expansion Ordinates.	Tension (Vacuum). Experiments without Vibration.		
	Increasing Load.	Decreasing Load.	Range.	Increasing Load.	Decreasing Load.	Range.			Increasing Load.	Decreasing Load.	Range.
$\frac{1}{12}$	−2·5	+2·1	4·7	+1·3	−0·2	1·5	..	+12·0	+0·4	..	..
	(−3·1	+2·5	4·6	when pencil kept continuously in contact).							
$\frac{1}{16}$ I.	−2·1	+4·6	6·7	+2·5	..	..	+2·3	+ 7·6	+0·8	+2·3	1·5
$\frac{1}{16}$ II.	−3·1	+3·1	6·2	+1·9	..	..	+1·2	+ 8·7	0·0	+0·8	0·8
$\frac{1}{20}$ I.	−1·3	+6·0	7·4	+3·0	+3·3	0·3	+3·0	+10·7	..	..	..
$\frac{1}{20}$ II.	−0·5	+5·3	5·8	+3·9	+2·6	1·3	..	+ 9·6	..	..	..
$\frac{1}{24}$ I.[1]	+1·6	+5·8	4·2	+5·0	..	..	+4·1	+10·3	..	..	..
$\frac{1}{24}$ II.[1]	+2·0	+6·3	4·3	+4·4	..	..	+5·0	+ 8·2	..	..	..
$\frac{1}{35}$ [2]	+0·9	+5·1	4·2	+3·9	+3·0	0·9	+3·3	+ 8·5	..	..	..
	(+0·5	+5·8	5·3	when pencil kept continuously in contact).							
$\frac{1}{45}$ [2]	−3·2	+2·7	5·9	+0·5	+1·2	0·7	−1·0	+ 5·2	..	..	..
	(−4·3	+3·9	8·2	with pencil continuously in contact).							
$\frac{1}{8}$ I.[3]	−2·5	−0·8	1·7	..	..	..	..	+ 7·7	−2·8	+0·6	3·4
$\frac{1}{8}$ II.	−3·3	+3·3	6·6	..	..	..	..	+ 9·3	−1·7	+2·0	3·7
$\frac{1}{8}$ III.	−0·8	+5·0	5·8	..	..	..	..	+14·0	0·0	+3·3	3·3
$\frac{1}{8}$ IV.[3]	−4·2	0·0	4·2	..	..	..	..	+ 3·7	−3·3	−1·8	1·5

Remarks.

[1] These two springs were tested in three different indicators (all Richards) without any difference in the results sensibly traceable to the change of instrument.

[2] These springs were tested with two different forms of (dead weight) gear, without any difference in results traceable to the change.

[3] This spring was tested in tension by Messrs. Donkin against a mercurial column, with practically identical results.

Professor Kennedy.

descending, as compared with its ascending positions, and the way in which the "admission-ordinates" (column 8 of Table I) and the "expansion-ordinates" (column 9 of Table I) were drawn.

TABLE II.

Length of String.	Number of Cards.	Length of Card.
Inches.		Inches.
9	51	4·11
24	51	4·02
30	51	4·09
48	51	4·04

TABLE III.

Number of Indicator.	4,526.	1,392.
Spring.	Percentage of Error.	
$\frac{1}{20}$	+3·9	+2·7
$\frac{1}{30}$	+7·8	+6·9
$\frac{1}{48}$	+4·8	+3·8

Mr. Longridge.

Mr. MICHAEL LONGRIDGE desired to compliment Professor Reynolds upon his Paper, the volume of which was quite disproportionate to the amount of thought and labour bestowed upon it. But he dissented from the general conclusions at which Professor Reynolds had arrived, and he should not like it to go abroad that engineers were putting so much faith in the indicator as had been alleged. The Author, however, had good excuse for what he had stated, because engineers were apt to keep technical difficulties very much to themselves—a course which he thought was a very wise one. He once went to a mill, and unfortunately got into discussion with the manager as to the pressure shown upon an indicator-diagram. He happened to speak of back-pressure in the case of diagrams from a condensing-engine, and the manager asserted that there was no such thing as pressure under atmosphere, and the engine-man was called in to corroborate the statement. Mr. Longridge left the mill with his own reputation and that of his company damaged, because he could not get them to understand an expression with which they were unfamiliar. It was best for engineers to keep such matters to themselves. In publishing engine-tests, &c., he had avoided going into detailing minutely the precautions which were necessary, and believed that by doing so he prevented much troublesome and useless criticism. A man who would take all the trouble required to go through the number of figures involved in engine-trials would be likely to make sure of the data as far as he could. Therefore it must not be assumed that because engineers said little they did not understand the difficulties attending the proper use of the indicator. Indicators were used for several purposes. One object was to ascertain the power developed by an engine, another was to examine the diagrams

Mr. Longridge. with a view to proper valve-setting, and another for scientific measurements. As to the first, he was convinced that the power was very nearly registered by the instrument as it stood. He could not speak as to locomotive-engines, because he had had no experience with them; but in the case of mill-engines, running from 30 to 60 revolutions per minute, he was convinced that the power, as recorded by the indicator, was very nearly correct. He was the possessor, officially, of about thirty thousand diagrams, which, he thought, ought to add some weight to his opinion. Many of the engine-tests that had been made agreed within 1 per cent. The heat-consumption, when measured from the indicator-diagrams and the feed to the boiler, or from the diagrams and the ejection-water, came often within 1 per cent. Those were independent checks upon each other, and such results could not be obtained if there were errors of anything like 20 per cent. in the diagrams. For valve-setting, the old direct-acting-indicator was a useful instrument in certain cases—old beam-engines, for instance—because the oscillations which were set up showed the suddenness or otherwise of the admission. When he used it for those engines, he was never satisfied unless he could reduce the vibrations to a very small amount. When the diagram was free from vibrations, he felt certain that the engine was getting over the centres easily. In that case the McNaught indicator was very useful, but the Richards indicator would not show the same thing. For high speeds, the Crosby indicator was better than the Richards. With reference to the piston not coming down to the same place as it started from Mr. Mair had mentioned the matter to him, and he was puzzled by it; but on thinking it over, it had occurred to him that when the Richards indicator was put upon a boiler to test the pressure-gauge, it would indicate nearly anything, while if it were put upon an engine running round and round, it would give the same diagram every time. He had got an indicator, and had tested it with dead weights, and he found that when the weights were put on and taken off very quietly, there was the difference that had been referred to; but when the weights were put on with a little jerk, and taken off suddenly, the indicator returned to the place from which it started. The slow and gradual reduction of pressure with the mercury column used by Mr. Mair would, he thought, be analogous to the quiet removal of the weights; but it was a question whether the rapid reduction of pressure during the expansion in the cylinder of an engine running at any speed would lead to the same error. With regard to the use of indicators for scientific purposes, he thought that the present instruments were rather inadequate. Two very important points in diagrams taken for such purposes were the compression-curves and points of release, and he had been under the impression that the stretching of the cord, and the inertia of the drum, affected those points. Mr. Mair had stated that that was not the case, and he was glad to hear it, because the considerations he had mentioned had thrown some doubts on experiments he had made. Another point was the difficulty of measuring small-scale diagrams accurately. It had occurred to him that the proper form of indicator would be a torsion-spring in the form of a straight bar, with a pencil to an arm at one end, so as to vibrate through angles proportional to the torsion of the bar. There would then be no lost motion, and the instrument could be made of a large size, and would give large diagrams, because the bar had a very small radius of gyration and very small moment of inertia. He had had some springs made, and had found that with a length of about 8 inches, and a lever at the end 3 inches long, he could get a diagram of the size of the ordinary Richards indicator-diagrams. With regard to the testing of springs, he should like to ask makers whether they tested them when hot. He had tested a good many springs when cold, and found that they were correct, and in that case he could not see how they could be right when they were hot. Mr. Longridge.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, President, said he did not know whether any speaker was about to mention the Bourdon indicator that was shown at the Great Exhibition of 1851. In that there was no piston, but merely the flexure of a tube under pressure. The instrument was invented with the object of having very little weight in the moving parts.

Mr. Mair.

Mr. J. G. MAIR had tried that indicator, but had abandoned its use. There was a greater difference between the going down and the coming up than with the ordinary Richards or Crosby indicator.

Mr. Cowper.

Mr. E. A. COWPER said that the friction was too great, making the vibrations very unpleasant.

Mr. Anderson.

Mr. WILLIAM ANDERSON observed that he was about to take exception to the poor opinion expressed by Professor Reynolds as to the capacity of engineers to appreciate the errors of the indicator; he would not, however, do so, but he would thank him sincerely for the manner in which he had drawn attention to the subject. He did not agree with the conclusions given in the Paper, because he did not think that Professor Reynolds had all the premises on which to found his calculations. Two or three sources of error appeared to have been altogether overlooked in the Paper; one of them had been referred to in the

Mr. Anderson. discussion, namely the uncertainty of the position which a perfectly free spring would assume according to the way in which the load was applied or taken off. A perfectly free spring, in which the only impediment to free action was the molecular resistance of the spring itself, that was to say, its imperfect elasticity, was such that it might be made, according to the way in which the weight was put on or taken off, to vary considerably in its indications with the same load. That was a very troublesome error in all testing-machines which depended upon springs. In the case, for example, of dynamometers used for measuring traction, it was necessary to test the springs over and over again, because they varied more or less from day to day. The difficulty described had been alluded to by Professor Kennedy, and it accounted for the errors spoken of by Mr. Mair, arising from the uncertainty of the position to which the spring returned. He did not see how any calculations could help them out of the difficulty, because while the indicator-diagram was being traced the velocity with which the load was applied and taken off was constantly varying. The other point—a very grave one, which he thought frequently absorbed all the others—was the error arising from the length of the pipes which intervened between the cylinder and the indicator. There were some engineers who thought it desirable to arrange an indicator in the middle of the cylinder and connect it by a pipe to each end, arranging a two-way cock so that they could bring either end of the cylinder into connection with the indicator. He believed that such was, at one time, the practice of the Admiralty, but he was not sure that it was so now. At any rate the practice was common, though it was evident that a certain loss of time took place before the pressure in the cylinder was communicated to the indicator, and that loss of time was very considerable. The evil was aggravated if the indicator was put close to one end of the cylinder having a long pipe from one end and a short one to the opposite extremity, so that the indicator-diagram from one end of the cylinder was distorted a great deal more than the one from the other. That was reprehensible, and he hoped it would never be sanctioned by any engineer who had listened to the discussion. Another cause of serious error (and he did not see how it was to be guarded against by calculation), was that there was always more or less water coming from the cylinder and interfering with the indicator-piston. It was not known what effect the weight of that water had upon the inertia of the piston, and what effect it might produce upon the indicator-cord. He had mentioned all those sources of error, not because he thought that those who were acquainted with the indicator needed to be told about them, but simply because he wished to show that in the Paper two or three considerable sources of error had been omitted, and it was impossible to say how far the mathematical correction which had been applied to some recognized errors really met the ends in view. It had been stated that the error arising from the stretching of the string was of little or no moment if the string was properly stretched. Of course, if the string was a new one, and had never been stretched before, the error might be almost anything; but with ordinary care the error from that source would not be a very serious one. Mr. Anderson.

Mr. Davey. Mr. HENRY DAVEY was much indebted to Professor Osborne Reynolds for pointing out the errors of indicators; but as a practical user of the indicator he was convinced that those errors had been greatly exaggerated. If they were as great as the Author would infer, engineers would often find themselves in this dilemma, that they would have an indicator-diagram from an engine which, when the assumed errors were discounted, would show actually less power than the work really done. Taking the case of a pumping-engine, which was often found to give as high an efficiency as 85 or 90 per cent., if the errors of the indicator were so enormous as had been suggested there would be less indicated power than actual power. When carefully used, he thought the indicator was a very trustworthy instrument. No doubt it was sometimes used without judgment, but in the majority of cases the liabilities to error were recognized, and pains were taken to eliminate the errors as far as possible.

Dr. Hopkinson. Dr. J. HOPKINSON said it was the duty of the indicator to record the relation between two quantities, one representing the position of the piston, and the other the pressure on the piston; and the accuracy of the record depended upon the accuracy with which those two elements were recorded. The errors in the record of the position of the piston arose, as Professor Reynolds had pointed out, from the stretching of the string, acting through or rendering possible the effect of the inertia of the drum and of the friction of the pencil upon the motion of the drum. Professor Reynolds had suggested that some important conclusions which had been drawn from the indicator-diagrams were due to that cause. In particular he suggested that possibly the apparent re-evaporation of condensed steam which the indicator-diagram showed to occur from the surface of the cylinders, was due to errors arising from the stretching of the string. Mr. Mair had pointed out that at all events the effect of those errors could be

Dr. Hopkinson. varied by indicating with a motion, not from the cross-head itself, but from some other part, for example, from the valve-motion. Supposing such a test as that were to show that there was a serious error, it appeared to him that the wiser course would be to eliminate the possibility of error from the stretching of the string by taking the indicator-diagram, with a continuously-moving paper. There was no difficulty in adapting the ordinary Richards indicator to do that. The result would be a diagram of modified form like Fig. 45, and there was no difficulty in reducing that to the normal diagram (Fig. 46). Then it would be certain that no errors would arise from the stretching of the string, or from the position of the drum. The only difficulty would be to record the precise position of the beginning of the stroke. One interesting point on which Professor Reynolds had dwelt was that the stretching of the string would, under some circumstances, cause a very flat end to the diagram. If the problem were worked out, he believed it could be shown that that flat end to the diagram might exist until a certain speed was attained. Below a certain speed

Fig. 45.

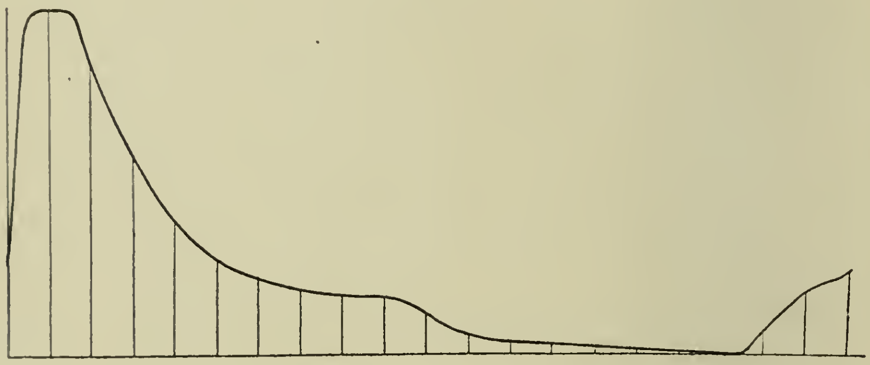
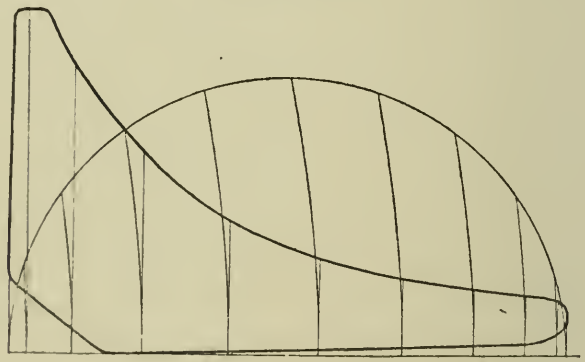


Fig. 46.



Dr. Hopkinson.

of the engine there might be, with a given extensibility of string, a flattening at the end of the diagram. After that the flattening would cease, and other errors would come in. Professor Reynolds had tried experiments in which the record of the true position of the piston was made by electric means through an induction-coil. No doubt the spark from an induction-coil would occur a certain small time after the break of contact was made. It would be very difficult with an ordinary induction-coil to calculate *à priori* how much that retardation amounted to. But he did not think it would be safe to assume, as beyond all doubt, that the time of passage of the spark was an absolutely precise record of the time at which the contact was broken. Professor Reynolds undoubtedly did prove by diagrams that there was a variation depending upon the extensibility of the string, because the effect was different in the case of a string, and in the case of a wire, but he did not think that proved that there remained a sensible effect when the wire was used. The effect of friction on the position of the pencil had been clearly pointed out in the Paper, and if it were followed it would show a perfectly definite curve. He thought there might be a little confusion arising from the kind of curve which Professor Reynolds had shown in Fig. 2. That curve did not come down to the actual datum-line, but taking Professor Reynolds' account it ought to come down there. No doubt the difference between the actual line and the datum-line was due to friction other than the friction between the pencil and the paper. A solution had been given of an equation with regard to the motion of the piston when there was no friction. That solution, he thought, was an approximate one only, and any one reading it might be a little confused by that fact not being pointed out. If there were no friction to damp the vibrations of the piston some curious effects would arise. There would be effects of resonance; and if the time of the vibrations of the piston happened to be commensurable with the time of the rotation of the engine, there might follow some extraordinary effects of vibrations being greatly exaggerated. Of course in practice those things did not happen, because friction existed, but the treatment of the question from a theoretical point of view—without the friction coming in, and without any statement that the solution was only an approximate one—was liable to be a little confusing. He agreed with Mr. Mair with regard to the method of testing indicator-springs. He felt some uncertainty as to whether the method adopted by Professor Reynolds was not subject to error. The pressure was

r. Hopkinson. applied to the piston of the indicator not in precisely the same way in which it was applied in practice. It was quite as easy, and much more scientific, to apply the pressure in exactly the same way, and with exactly the same temperature, by attaching the indicator to a boiler with throttled steam, till variable pressures were obtained, and then to measure them in the way indicated by Mr. Mair. It was not necessary to employ a single long column of mercury. A series of columns might be used, allowing one column to bring pressure upon the next. The dynamometer selected by Mr. Brightmore had, he thought, been first used by Sir Frederick Bramwell. One very similar to it was used by Dr. Hopkinson in cases in which the power was rapidly varying, as in the case of a gas-engine, or hot-air engine. The employment of a weight was then somewhat inconvenient; its inertia was apt to cause considerable oscillations, which introduced a considerable amount of error; and the use of the dash-pot to damp those oscillations was also possibly the source of other errors. In the experiments he had made upon gas-engines, he had found it expedient to use an ordinary powerful spiral spring, which might be readily measured independently of the extension of that spring, and of the counter-poising spring shown in Fig. 13. In that case, though the motion, of course, was more or less irregular, and the actual power developed was also irregular, the spring was quite steady, and it was easy to measure its extension with a considerable degree of accuracy. In fact, he had no doubt that the error of the dynamometer-tests made by him up to 19 or 20 HP. would not be so much as 1 per cent.

Mr. Wingfield. Mr. C. H. WINGFIELD observed that the force due to inertia, acting alternately with or against the direction of motion of the string of an indicator, varied from nothing at the centre of the stroke to a maximum at each end. In Fig. 47, A B represented the direction of the string and length of its travel. The indicator-drum was shown as if connected directly with the piston-rod cross-head, for the sake of distinctness, but it was to be understood that some form of reducing-gear was applied. Let the maximum pulling or slackening force, due to acceleration or retardation of the drum, be represented by the vertical ordinates B D and A C. Join C D by a straight line passing through the centre O. If the effect of the obliquity of the connecting-rod were neglected, vertical ordinates drawn from O B to O D would represent + forces, or forces tending to tighten the string at each point of its travel, and similar ordinates from O A to O B would measure − forces, or forces which tended to slacken the string.

Mr. Wingfield.

The signs + and − would be on the same side of the figure, in whichever direction motion took place. He would not estimate the exact amount of these forces, as they would vary for each indicator with its weight, the diameter of the drum-pulley, &c.; but would point out that, having found the value of A C for one speed, its value for any other speed of the same engine and the same indicator would vary as the square of the revolutions of the engine per minute.

Taking a Richards spring, purposely made very long so as to give an approximately uniform pull throughout the revolution of the drum, its action might be represented by a line E F Z, cutting C D produced in Z (Fig. 48). The point Z was that part of the stroke which would, if the stroke were sufficiently lengthened, allow the spring to be quite unwound. D F represented the pull due to the spring at one end of the stroke and C E that at the other end, and the more constant the pull of the spring, the more nearly would E Z be parallel to C Z. The force D B helped to tighten the string, and brought the total pull on it up to B F at one end, and the "slackening force" A C diminished the pull to A E at the other end of the stroke. Vertical ordinates erected on A B and terminating on E F would be measures of the tension on the string at each point of the stroke, due to the combined effect

Fig. 47.

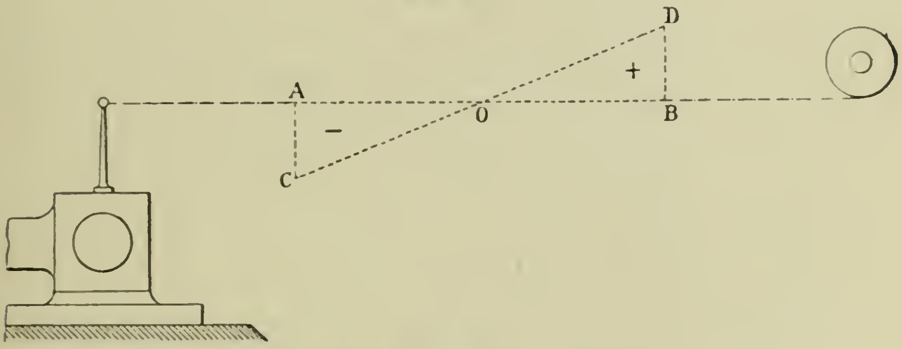
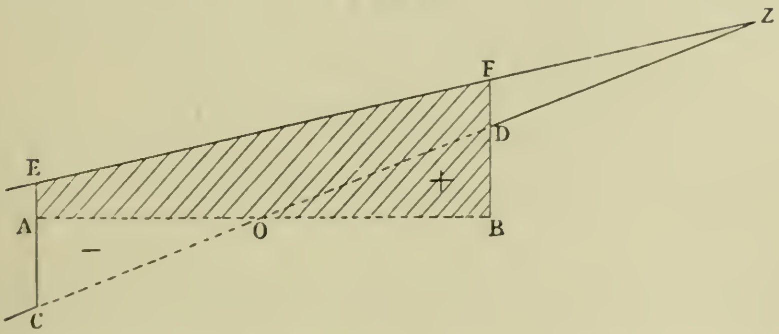


Fig. 48.



r. Wingfield. of the inertia of the drum and the tension of the spring. It would be seen from an inspection of Fig. 48 that the string was more tightly stretched at B than at A, the tension varying with the width of shaded area, and that the more nearly the condition of uniform resistance of spring was carried into practice the greater was the variation of the tension of the spring. By shortening the spring till its pull increased at such a rate that, when fully coiled, its strength C E (Fig. 49) = B D + A C = 2 B D, and its pull at D = zero, there resulted a constant tension = D B throughout the stroke, as shown by the uniform width of the shaded part of the diagram.

From practical considerations it was necessary to have an initial stress on the string, and this altered the diagram to Fig. 50, which showed an initial pull due to the spring = D F, and an absolutely uniform tension on the string = B F throughout the stroke. This condition had been realized by the Crosby Steam-gauge Co. as far as this particular source of error was concerned. As he should have to refer particularly to this company's catalogue, he might here state that he had no interest of any kind in its manufactures. In his opening remarks Professor Reynolds said that he was not aware that the effect of the stretching of the cord had been noticed. In the early catalogue of the Crosby Steam-gauge Co. it was pointed out that if there were variations in the stress, the cord would stretch, the motion of the drum would not coincide with that of the piston, and that errors would appear in the card.

FIG. 49.

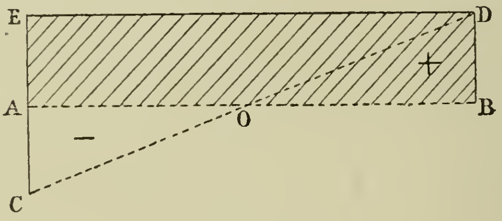
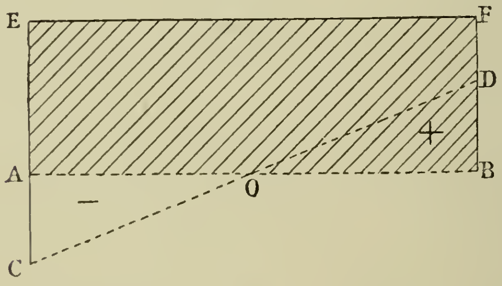


FIG. 50.

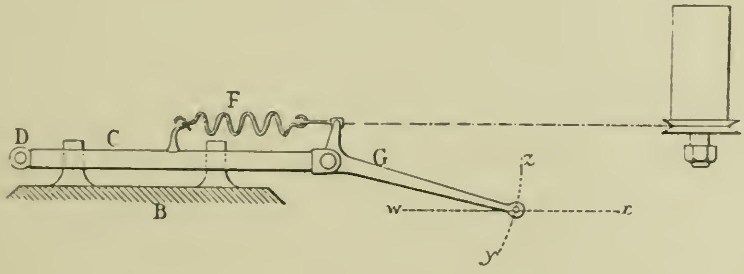


Mr. Wingfield.

It was then pointed out that a long spring was wrong in principle, and further that "if the conditions under which the spring is used be considered, it will be readily seen that when the cord has the most other resistances to overcome, the drum-spring should offer less resistance than at any other time. At the beginning of the stroke in the other direction, however, when the spring is overcoming the inertia and friction of the drum, its resistance should be greatest, and should gradually decrease as the work it has to do decreases. It was owing to these facts that in the design of the Crosby indicator the short spiral drum-spring was adopted."

Starting with this idea he had worked out the results shown in Figs. 47 to 50, which showed that Messrs. Crosby were correct. An instrument was used by them for testing the stresses on indicator-cords, of which the principle was exhibited in Fig. 51. A bar C was made to slide in two guides, and, by means of a crank and connecting-rod, of the same relative proportions as those of the engine with which it was proposed to use the indicator, jointed at D, it was made to reciprocate at the rate at which it was intended the piston of the engine should act. A spring F had one end attached to C, the other being connected with the drum to be tested by a string of the same length and material as that in ordinary use. A bell-cranked lever G was hinged on C, the short arm being made to move with the free end of the spring. If the tension of the string increased, obviously the long arm of G would move towards y, and if it diminished, the motion of G would be towards z. If the bar C, and the lever G reciprocated without any alteration of the tension of string, a pencil at the end of G would describe a straight line w x on a piece of paper held against it parallel to the plane of this book. Deviations above or below this line would indicate corresponding changes in the tension of the string. In the actual instrument, the lever G was so placed that downward movements indicated diminution of tension and *vice versâ*. Fig. 50 represented a uniform tension on

Fig. 51.



Mr. Wingfield. the cord when the engine was running at a certain speed. If this speed was doubled, the lines A C and D B were quadrupled, and a similar diagram to Fig. 48 would be produced. This proved that the indicator could only be adjusted to give best results for one speed, and as the error increased as the square of the speed, and therefore more rapidly at higher speeds than at lower ones, in an instrument intended for use in engines running from, say 400 to 600 revolutions per minute, the spring should not be adjusted to 500 revolutions, but to a slightly higher speed. The principal effect of the connecting-rod would be to render the line O D (Figs. 47 to 50) convex towards B and O C curved towards A. As the ordinates D F, A E, &c., due to the spring remained unaltered in length, the line E F became a sinuous curve; but the error need never be great, and at the three points A, O, and B, the tension should still be nearly equal. It might be worth while for manufacturers to consider the feasibility of an arrangement for shortening the drum-spring similar to the regulating-mechanism of a watch. If this could be carried out, the same instrument could be used for quick or slow speeds by simply setting an index to the speed required. Some little time ago he found signs of sticking in an indicator which appeared to be free from dirt. On the supposition that, as the piston was always exposed to steam and the top end of the cylinder never, the consequent expansion of the former might make it too large for its cylinder, he recommended that the cylinder should be lagged. It would be interesting to hear why this was done in the case instanced by Mr. Rich, and with what result. In corroboration of Mr. Mair's remarks, he might say that in indicating engines making 600 revolutions per minute and more, he had found it possible to obtain fair cards with either the Darke or the Crosby indicator by using sufficiently stiff piston-springs to minimize the vibrations. In these engines it had been found most convenient to take the cross-cards mentioned by Mr. Mair as giving the correct points of distribution, by obtaining the motion for the high-pressure indicator from the low-pressure cylinder, and *vice versâ*, instead of from the valve-motion. If it were attempted to reduce the pencil vibrations with such speeds by pressing the pencil against the paper as suggested by the Author, there would be little paper left to press against.

Professor Reynolds. Professor REYNOLDS, in reply upon the discussion, said that nothing had been further from his or Mr. Brightmore's intention than to find fault with the indicator, or to express a poor opinion of the capacity of engineers to appreciate its errors; and he felt sure that some of the speakers, particularly Mr. Cowper, Mr. Mair, and Mr.

Professor Reynolds.

Harris, would on a more careful perusal of the Paper, see that they had greatly misunderstood its purpose and its points. It was not stated in the Paper that the effect of the stretching of the cord had never been noticed before: but that "This effect" (that of distorting the diagram) "appears" "to have been so entirely unperceived." This was borne out by Mr. Mair's remarks, in which he missed the point. Had he not perceived the importance of distortion, he would not have entered into the question of the slope of the lines in the diagrams from Brown's instrument for testing the tension of the cord at different parts of the stroke; which slope merely showed the different tension in the cord at the ends of the stroke due to inertia and variation of spring-tension; for at the conclusion of paragraph 4, page 12 in the Paper, it was stated that the stretching due to these causes, although it might affect the length of the diagram, did not affect the mean indicated pressure. In this paragraph it was also shown that for a particular speed of the engine these two effects would compensate each other; and the particular speed given for the Richards indicator, 150 revolutions, seemed to correspond very well with the speed at which the line, as shown in the diagrams furnished by Mr. Mair, should have become horizontal. What was pointed out in the Paper, and what Mr. Mair did not seem to have perceived, was that it was the difference in tension (and consequent stretching) in the forward and backward strokes which diminished the mean indicated pressure. Had Mr. Mair noticed this, he certainly would not have brought forward the diagrams taken with Brown's instrument as contravening the Paper, and this without stating which of the two lines shown at each speed corresponded to the backward and which to the forward stroke. If anything was wanted to show the need that the importance of this effect should be pointed out, even to those who had looked into the indicator, it was this part of Mr. Mair's remarks—for the very diagrams by which he intended to demonstrate that the tension of the cord had been fully studied in America showed, by the distance between the two lines, as great a difference in the mean tension in the forward and backward stroke, not only in the Richards but in the Crosby, Tabor and Thompson instruments, as that required for the distortion of the diagrams given in the Papers.

These diagrams of Mr. Mair's showed at certain speeds a difference, in the forward and backward stroke, of about one-third the gross tension. This with the Richards indicator amounted to 1·5 lb., which difference of tension would be more than sufficient to cause an alteration of $\frac{3}{8}$ inch in a cord 9 feet long

Professor Reynolds. such as those supplied with indicators and ordinarily used in indicating; and yet this was all that was necessary to restore Fig. 11, with a difference of 16 per cent. in the mean pressure. In calling attention to the fact that these diagrams from Brown's instrument should emphatically have the very effect Mr. Mair had not perceived, he would like to say one word of caution about this instrument of Brown's. He had not examined it, but until he had seen it tested he should have doubts as to whether it might not itself be liable to errors similar to those of the indicator, particularly by the modification of the motion of the drum by the spring. As regarded the oscillation, although users of the indicator might pride themselves on being able to take diagrams which did not show oscillations, his own experience was that if a diagram was to be relied on it ought to show oscillations however small, because springs if free always did oscillate when disturbed; and if the oscillations were not shown it was a proof that they had been eliminated by friction, or as Mr. Cowper had said, by the aid of the thumb of a "skilful operator." He had not accused engineers of pressing the pencil home. He had merely stated that the friction of the pencil might take off the oscillations, a fact of the utmost importance, because as it was possible to increase the indicated pressure by 5 per cent. by pressing the pencil, the existence of a due amount of oscillation according to the speed and shape of the diagram was very important, as showing that the pencil had not been so pressed. With respect to the stretching of the cords, he might repeat the statement in the Paper that many cords and wires had been tried. The length of the diagrams afforded uncertain evidence of the amount of stretching of the cord, unless the lengths of the diagrams taken at very different speeds were compared. At a speed at which the several effects of inertia, varying tensions, and friction balanced each other for a short cord, they would balance each other for a long one—as far as the length of the diagram went, although the effect of friction to shorten the diagram might be considerable, reducing the mean indicated pressure; while the effect of inertia to restore the length of the diagram would not restore the indicated pressure. Compare the diagrams taken with cord and with wire, with considerable expansion and with the same load on the engine. This latter condition was generally difficult, but if a definite point could be fixed, such as cut-off, the effect of friction would render this different according to the degree to which the cord could stretch. But the most satisfactory plan was doubtless that of pricking holes in the card by electricity. Dr. Hopkinson had objected to this method on account of the lagging in the passage

Professor Reynolds.

of the spark. But each lagging was a very small quantity. Since no condenser was used the spark passed between rupture of contact and the separation of the surfaces of the contact-breaker by a very small fraction of a millimetre; so that the ratio of this fraction to the length of the stroke would give the outside ratio of the time of lagging of the spark to that of a semi-revolution, and this was too small to produce any appreciable effect. Besides which, this was proved by the fact that with wire the points came very nearly opposite one another. Was the distortion shown by those pricked holes with string and wire a fair criticism of the indicator? It was an experimental result obtained with an indicator in exceptionally good condition. Both he and Mr. Brightmore had heard with the greatest interest the remarks of Mr. Rich, Professor Cotterill, Professor Kennedy, Mr. Longridge, and others, which contained many points for thought. He thanked the members for the interest they had taken in the Papers, which, as he was only too well aware, were by no means so complete as they might have been had their Authors had more time and facilities in their preparation. As it was Mr. Brightmore had devoted six months to the experiments, assisted by Mr. Foster of Owens College, who for several years was an inspector of steam-boilers under Mr. Longridge.

Mr. Brightmore.

Mr. A. W. BRIGHTMORE, in reply to the query why a somewhat complicated apparatus should be used to indicate the speed, observed that the apparatus described was employed on account of its simplicity, and the definiteness of its action. An impression seemed to prevail that in the experiments the indicator was some distance from the engine. This was not so; it was early found that throttling had an important effect in reducing the size of a diagram; care was therefore taken in every case to have the indicator screwed directly on the end of the cylinder from which a diagram was required. Alluding to observations as to the precautions which should be taken in applying the indicator, he might state, that whatever his experience in manipulating the indicator might have been before commencing the experiments, he had, during their continuance, the valuable assistance of Mr. Foster, who was for many years an inspector for one of the Boiler Insurance Companies. He might also state that the indicator was of the most modern type of Richards, and in excellent condition, and that, owing to the care bestowed, it was probably in a better state when the diagrams were taken than 99 per cent. of those in use.

Concerning the remark as to the oscillations due to the spring

Mr. Brightmore. not being strong enough, and to the inertia of the moving parts, being well understood by engineers of experience, this was not doubted, though that the remark would not apply to all who took indicator diagrams would be seen by reference to "The Engineer" of the 6th of November, 1885; in which four diagrams from a gas-engine were given, showing little else but the oscillations of the spring. In describing these the writer said:—"They are interesting as characteristic diagrams, but much of the apparent fluctuation of pressure is no doubt due to the indicator."[1]

In answer to Mr. J. G. Mair, it was not supposed that springs had never been experimented upon before, nor was the apparatus employed recommended as a rough and ready method to be used by engineers in testing springs, but it was necessary to know exactly the errors of the springs used, so as to be able to allow for them in working out the results of the further experiments, and the method explained was devised as an accurate one for the purpose. He had no idea of attempting to free the engine used in the experiments from the contempt with which it had been referred to by one of the speakers, but he contended that the results were of some value. There was no intention to impute any important effect in modifying the area of a diagram to the oscillations, but rather in disfiguring it. With regard to the temperature of indicator-springs, it would be seen, by referring to Table III (p. 27), that the percentage error in the fifth column, as explained on p. 24, was the difference between the deflection as calculated from the mark on the spring, and the deflection as found by experiment on the springs at the temperature of 212° Fahrenheit.

Referring to Professor Kennedy's remarks on the constancy of the lengths of diagrams taken with string, the same fact was brought out in the experiments, Table VII, though, on comparing these diagrams with others taken with wire, discrepancies exceeding 6 per cent. had been noticed. As to the objection that the errors of indicator-springs might be much greater than stated in the Paper, and that after applying a dead load to a spring, and then removing it, the spring would not return to its original position, the results were probably due to friction. He had endeavoured to separate each cause of error, hence in the experiments on the deflection of springs it was necessary to get rid of friction. This was effected by tapping the frame which supported the indicator with a light hammer each time a weight was added

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[1] "The Engineer," vol. lx., p. 360.

Mr. Brightmore.

or removed. By these means perfectly definite results were obtained both on adding and subtracting weights, although it was his opinion after carrying out these experiments, which were repeated many times both when the spring was hot and when cold, that it never deflected exactly the same way twice over. All error due to the parallel-motion was got rid of by taking the readings direct from a fine scratch on the indicator-piston.

The effects of the errors pointed out had been entirely misapprehended by Mr. Davey when he said that, if they existed to the extent stated, the indicated power of a pumping-engine would be less than the actual power. Of the errors commented upon, those tending to increase the size of the diagram were due to the cyclical effect of the inertia of the moving parts, and to the friction of the pencil and mechanism; but these were shown to be small compared with the error due to the friction of the drum. This entailed, as explained, a decrease in the size of the diagram, so causing the efficiency of an engine to appear greater than it really was, instead of less.

That the lag of the induced current referred to by Dr. Hopkinson, after the breaking of contact, was very small was shown by the fact that, after allowing for the thickness of the wire in the contact-breaker, the holes, corresponding to the extreme positions of the piston, were always pricked at the ends of the diagram, although the breaking of contact, in these cases, would be effected with less rapidity than for the other nine positions. He might mention, in conclusion, that very little difficulty was experienced with the friction-brake, except at certain critical velocities, when the time of oscillation was commensurate with the period of revolution of the engine; the oscillations then had to be damped before commencing the experiment.

Correspondence.

Mr. Barrus.

Mr. G. H. Barrus, of Boston, U.S.A., considered that the Papers furnished a complete exposition of the nature of the errors to which indicators were liable, the manner in which they operated, and the extent to which in some cases they acted. It could not be doubted that the friction of the pencil upon the card, the inertia and friction of the pencil-mechanism, the stretch of the driving-cord, and the inertia and friction of the drum-mechanism, all acted to distort the diagram and make it more or less erroneous. It might be doubted, however, whether the conclusions as to the

Mr. Barrus. amount of distortion which these defects produced were applicable to good practice. These conclusions were based on investigations made with a Richards indicator. In this instrument the moving parts were all unnecessarily heavy. Not only was this true of the pencil-mechanism with its arrangement of parallel bars, but it was also true of the drum-mechanism. The Richards indicator had been superseded, at least in the United States, by instruments which possessed these defects in a much less degree, and it was to be regretted that the investigations were not made on one of these instruments, so as to bring the results into accord with the latest practice. This was the only adverse criticism he had to offer.

Unnecessary weight in the moving parts appeared to be the cause of the principal errors to which indicators were liable. Since the time of the Richards indicator, nearly all the improvements had been in the direction of overcoming this defect, and it was in this field principally that further improvement might be expected. Much had already been accomplished. A comparison of the weights of the Richards indicator with those of one of the prominent modern instruments having the same size of drum, showed that a marked advance had been made. In the one, the weight of the several bars, which carried the pencil, was 11·3 grams (174 grs. troy); in the other, the single pencil-bar weighed 1·8 gram (28 grs.). In the one the weight of the drum-mechanism was 594 grams (9,166 grs.); in the other 207 grams (3,088 grs.).

Further than this, it was probable that little reduction in weight could be effected without a change of proportions, or a change in the metal of which the parts were constructed. Something had been accomplished in the first direction by reducing the diameter of the paper-drum, and this was on the whole an improvement. Changes of this nature reduced the size of the diagram, which was undesirable, and consequently improvement in this direction was limited. Little had been done in the second direction, except in the line of experiment; but there was a field here for much progress. The metal aluminium furnished a material for greatly reducing the weight of nearly all the moving parts, and its use for this purpose promised excellent results. He had experimented with it when used for piston and piston-rod, and had compared its action with that of the composition ordinarily adopted. The diagram taken with aluminium showed that some of the oscillations of the pencil were entirely suppressed, and the amplitude of the remaining ones was reduced one-half. The result was a diagram nearly free from wavy lines at a speed of 310 revolutions per minute. The weight of the aluminium piston and rod was 4 grams

Mr. Barrus.

(62 grs.), while that of the same parts made of composition was 14·4 grams (222 grs.). Although aluminium had not to his knowledge been employed for other parts, he knew of no reason why it could not be used for the whole pencil-movement, excepting the wire of the spring, and for the drum-mechanism as well. If this were done the indicator would become an almost perfect instrument. Even in the present condition of the best instruments, the investigations and observations that he had been able to make on the subject, led him to conclude that in competent hands their combined errors amounted to only a small percentage.

Finally Mr. Barrus drew attention to a Paper by him on a "Comparison of Three Types of Modern Indicators,"[1] which treated of some of the errors of indicators.

Mr. Beaumont.

Mr. W. W. Beaumont observed that, although Professor Reynolds had given an instructive digest of the experiments described, he seemed to have been unaware of the extent to which the causes of the errors of which he complained had been already removed in modern indicators, and that the Richards indicator was not now made by Messrs. Elliott, except by special order. The inaccuracy assigned, in the absence of another apparent cause, to drum-friction, must certainly be attributable to other sources, otherwise it would appear that drum-friction caused greater stress upon the string than the drum-spring, which could not be admitted. It appeared to be much more probable that the inaccuracy was the result of the springing of the vibrating rod due to its inertia, and, to a smaller extent, to slackness in the top guide. The former cause of inaccuracy had not been observed by Professor Reynolds; neither had he remarked upon the inaccuracy due to the want of rigidity in the fixture of the indicator itself. For high speeds, an arrangement for carrying the indicator-card similar to that used by Watt, but with rigid rod-connections between it and the engine, would give better results than a drum and flexible connection.

Messrs. Donkin and Salter.

Messrs. B. Donkin, Jun., and F. Salter, observed that the question whether long connections between the indicator and the cylinder would cause an appreciable error in the diagrams, had been put to a practical test by them eleven years ago in the following manner. The top of the low-pressure cylinder of a beam-engine was provided with two indicators; one indicator was placed on a short connection about 8 inches long, the other on a pipe of such length as would have been used if both ends

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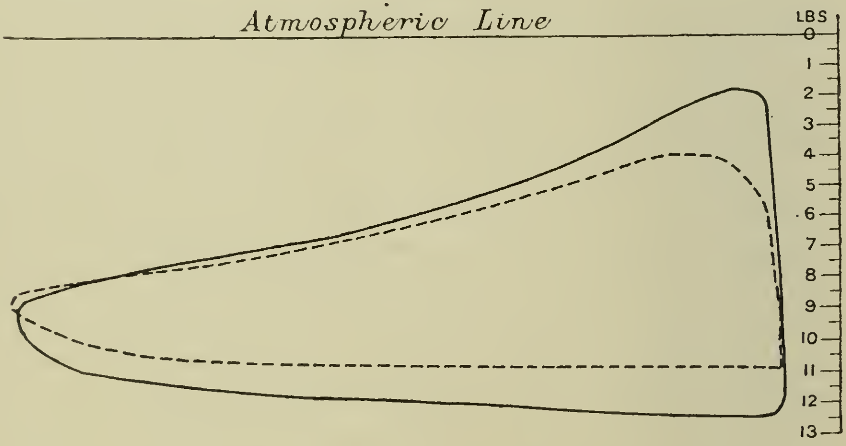
[1] Transactions of the American Society of Mechanical Engineers, 1883-4, vol. v., p. 310.

Iessrs. Donkin nd Salter.

of this cylinder had been in communication with one indicator, namely, 5 feet 6 inches long. Diagrams were taken simultaneously with these two indicators for a single revolution of the engine. The diagram from the indicator on the long pipe was found to be smaller than that from the indicator on the short pipe by from 15 to 30 per cent. of the latter. This difference was increased the greater the power exerted. The smaller diagram was deficient both on the steam- and on the exhaust-side, the steam-pressure shown being lower in the forward stroke, and the back-pressure higher in the return stroke, than that given by the diagrams with the short pipe. The two diagrams taken simultaneously in this way were shown by Fig. 52; the full line being the one taken on the short pipe, and the dotted line the one with the long pipe. The difference of course was not often as much as this; but that it even might be was sufficient reason, in their opinion, to condemn long pipes. In order to throw some light, if possible, on the causes of this loss with long pipes, the effect was tried of clothing the outside of this pipe thickly with cotton-waste. This produced, however, no improvement that could be detected. The pipe was then jacketed with steam. The result was that the diagram from the short unjacketed pipe, and from the long steam-jacketed one became identical. It might be added, that to eliminate small errors of springs, &c., as far as possible, the positions of the two indicators were frequently reversed as well as the operators. The ends of steam-cylinders were frequently joined with long pipes, and in their opinion this practice should certainly be discontinued where accurate results were required, and the shortest and most direct pipes used, and these latter should not be too small in diameter. It had been

Fig. 52.



Messrs. Donkin and Salter.

their practice to test indicator-springs when hot, and to correct them as they came from the makers. They had often been discovered wrong. All springs should be tested in the indicator, and when hot. They had always tried to reduce to a minimum the length of the cords, by the use of wire or rods, except around the indicator-barrels. They wished to direct special attention to the fact that the barrel of the indicator was frequently driven at a speed not bearing a constant proportion to the speed of the piston of the engine. The diagram was thus consequently falsified, and the error was often considerable. Wrong methods of driving were not only common in practice, but were advocated in works on the indicator, and in other Papers. Finally they recommended that the holes in indicator-cylinder covers should be made much larger than usual, say two of $\frac{3}{16}$-inch diameter, as with smaller ones the pressure on the top side of the indicator-piston often differed appreciably from atmospheric-pressure.

Mr. Halpin.

Mr. DRUITT HALPIN considered that Professor Osborne Reynolds' argument went to show that the relation between the brake and the indicated HP. of steam-engines was higher than that generally supposed, and deduced from indicator-diagrams as at present taken. The difference of the brake and indicated work was given as varying between 5 and 20 per cent., the former figure representing an efficiency Mr. Halpin had never before heard of. The highest efficiency developed by the engines tested by the Royal Agricultural Society, Cardiff, in 1872, was only 82·6 per cent., and it must of course be borne in mind that this engine was very carefully prepared for trial. As the engine with which Professor Reynolds' experiments were carried out was, according to his own showing, of a very ordinary type, it was to be hoped, if this very high result could be substantiated, some explanation of the cause might be supplied by the Author. One of the most serious causes of inaccuracy in the indicator arose from the inertia of the moving parts due to the weight multiplied by the velocity. Mr. Halpin considered a good deal of this error might be eliminated by the total suppression of the mechanism now used for enlarging the vertical scale of the diagram. If the pencil were attached directly to the top of the present spring, the inertia would be reduced to a minimum, the vertical scale of the diagram being then, however, only one-fourth of what it ordinarily was at present; this need not necessarily produce any inconvenience, as diagrams were practically only used for two purposes; viz., either for recording the distribution of the steam produced by the valve-gear, or for determining the mean pressure on the piston by means of a large

Mr. Halpin. number of observations, say as when a test of an engine lasting over several hours was being made. If it was desired to observe accurately steam-distribution on one of these diagrams of reduced vertical height, the vertical scale could readily be magnified to any desired height by means of the pantagraph, the error in using which would probably be much less than that produced when the multiplication of the vertical scale was automatically effected by the indicator itself, owing to the absence of inertia, and to the possibility of making the pin of the instrument of such dimensions as to eliminate the errors due to slackness. Should it, on the other hand, be wished merely to deduce the average pressure from a series of diagrams during an engine trial, this could be readily done by means of the Amsler planimeter, provided with locking-gear, enabling the instrument to perform the summation of the area of the diagrams, thus allowing the mean pressure to be calculated.[1]

Ir. Isherwood. Mr. B. F. Isherwood, Chief Engineer U.S.N., considered that only the inherent faults of the indicator, the ineradicable faults incident to its design and use, should have been dealt with in the Paper. The faults due purely to bad workmanship, to careless gauging, to improper attachments, or to unskilful use, were not faults of the indicator itself; and, if they were to be supposed as existing always, then, they would, for different instruments, exist in widely varying degrees, and an investigation of the amount of error given by any one particular specimen, would be true for that one specimen only, and have no application to others.

A considerable portion of the Paper by Professor Reynolds consisted of an exposition of the errors of the indicator, due to the stretching of its string, and the general tone assumed that this string always did and must stretch, and to an extent that caused marked error. No doubt improper and easily stretching strings were often used by careless or ignorant operators, but there was no necessity for it, and he had been accustomed always to see the engineers in the American Navy carefully select strings of very large and hard-twisted hemp-cord, or of good cat-gut, and to take out of them what little stretch there might be by suspending a considerable weight from them and allowing it to remain suspended a long time. By this means the elasticity of the strings was almost wholly destroyed, while their pliability remained. The pull of the spring of the indicator-drum upon the string was but little comparably with the strength and rigidity that it was possible to give

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[1] Minutes of Proceedings Inst. C.E., vol. lxxxii., p. 150.

Mr. Isherwood.

to the string. If the strain on the string were uniform, the diagram would not be affected by the stretching, consequently any error due to stretching would be only in relation to the variation in the stretching, caused by the difference between the maximum and minimum strains produced by the spring in its different positions. In using the indicator it was always allowed to work a little while before the diagram was taken, to ensure equilibrium of temperature, friction, &c., so that the strain of the spring was always upon the string during the taking of the entire diagram, and at the commencement of the taking. The pencil was applied to the paper while the drum was in motion, and was allowed to trace over the diagram several times. With proper care in the selection and preparation of a string, no sensible error would follow its use. Of course the operator was supposed to know the conditions of the instrument, and to employ it properly with intelligence and care. No instrument of any kind would give accurate results in incompetent or careless hands.

The friction of the piston of the indicator had not been distinguished by the Author, although this was the principal friction of the instrument; for the friction of the pencil on the paper was but trifling with a proper pencil composed wholly of graphite (no wood) pressed just sufficiently against the paper to make a barely visible line. The friction of the indicator-piston and of the pencil, either separate or combined, might be measured by attaching the instrument to the steam-room of the boiler, or to any place in which there was a constant pressure of steam; and, also similarly attaching by its side an accurate mercurial or other pressure-gauge. Now, supposing the spring of the indicator to be accurately indexed for the temperature, the difference between the pressure shown by the indicator and by the gauge would be the friction of the piston and of the pencil of the former, and of the mechanism carrying the pencil, provided the point of the latter was in contact with the paper. By removing the pencil, the friction of the other parts would be obtained alone.

No notice had been taken of the very important fact, that the piston of the indicator was subjected to widely-varying temperatures, which, consequently, by changing its area, made the scale or index of the piston-spring correspondingly erroneous. The error due to this cause was inseparable from the instrument, and constituted its greatest fault. By graduating the piston-spring according to the area of the piston at the mean temperature to which the latter was subjected, this error would be reduced to very little, so far as the area of the diagram was concerned; but

Mr. Isherwood. this mean temperature could not be known, and it changed with every variation of initial pressure in the steam-cylinder, and of back-pressure therein. In the case of a compound-engine, if the same indicator were used for both cylinders, the diagram from one or the other steam-cylinder, or from both, would certainly be wrong, because the piston of the indicator would have different areas when applied to the different cylinders, on account of their very different mean temperatures. Indicators should be manufactured expressly for the high-pressure cylinders of compound-engines, and expressly for the low-pressure cylinders. An essay which omitted the influence of temperature on the area of the pistons of indicators must be lacking in the most important point.

Although Professor Reynolds had not investigated the influence of temperature on the area of the piston of the indicator, he had on the spring of the piston. The temperature of this spring had been taken at 212° Fahrenheit. It was more than that. One end of the spring was in contact with the piston, and received heat by conduction, and the entire spring was surrounded by a brass barrel at nearly the mean temperature of the piston, which barrel radiated heat upon the spring. He considered 220° Fahrenheit to be the least possible temperature of the spring, and he had no doubt that in many cases it was higher. The temperature of the high-pressure steam leaking past the indicator-piston, was much more than 212°, although it had only the atmospheric pressure on emerging. It was super-heated, and had a temperature between 212° and that of the steam beneath the piston. The spring of the piston was directly acted upon by this outleaking steam, which must necessarily influence the temperature of the spring, as such leakage, owing to the construction of the piston, must always exist.

Again, Professor Reynolds had considered the effect of the inertia of the moving parts of the indicator and the effect of the friction of those parts as though these forces acted separately, whereas in fact, they acted conjointly, modifying, and, indeed, to a large extent, neutralizing each other, so that only the final difference of their effects was to be estimated, and not the sum in determining the influence they exerted upon the accuracy of the instrument. All the causes of error acted simultaneously, and the momentary result of their combined action was what was required to be known. Their independent values were, practically, comparatively unimportant, for they frequently neutralized each other in whole or in part.

Mr. de Maupeou. Mr. L. de Maupeou remarked that in France, as in England, great importance was attached to the indicator. One of the principal

Mr. de Maupeou.

tests in the trial of engines before acceptance consisted generally in the development of a specified HP.; sometimes even a premium was given for power in excess of the contract, and the indicator was generally sole arbiter in the matter. In the Mémorial du Génie Maritime for 1882 Mr. de Maupeou published a Paper on indicators,[1] in which their operation was studied in detail, and which presented more than one point in common with the notable theory of Professor Reynolds. It might, perhaps, not be without interest to note wherein their memoirs differed. As regarded the movement of the pencil, his Paper stated that the pencil of the Richards indicator did not describe a trace strictly perpendicular to the atmospheric line; and that the Martin indicator, furnished with an Oliver-Evans parallelogram, was superior in this respect. It insisted also on the testing of the spring, an operation usually performed in the presence of those interested when it related to the trials of the power of an engine. A test-curve would show that the scale of indicator springs was not generally the same in extension as in compression. This fact had been noticed in 1874 by Professor Berndt, of Chemnitz, in a long essay on the behaviour of springs under the influence of cold and heat.[2] Lastly, the parallelogram, by multiplying the strokes of the piston, tended to modify it. It was not sufficient to be satisfied that the flexures of the spring were proportional to the steam-pressure. The essential point was that the paths of the pencil-point satisfied this condition. In the Paper in the Mémorial the oscillation of the pencil was considered by a less common, but perhaps more practical method than that used by the Author. In the first place, the cause of these vibrations was shown, and special curves were used to give an idea of the rapidity with which the steam acted on the piston. Further, the diagram itself showed the moments when the excess of the motive power was stored by the moving parts in the state of *vis viva*, and from this was drawn the conclusion also arrived at by the Author, that at the moment of admission, if the pressure of the steam acted instantaneously, and the friction and other causes of loss of *vis viva* were neglected, then the steam "forces the pencil twice as far as it ought to go." Such was the origin of the evil. It might be added that by slowing, without excess, the ascensional movement of the pencil, these vibrations could be diminished without sensibly altering the form of the curves. The valve of the Martin indicator, which throttled the steam during the period

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[1] Abstracted in Minutes of Proceedings Inst. C.E., vol. lxxii., p. 386.

[2] "Engineering," 1877 and 1878.

Mr. de Maupeou. of admission, and opened during the period of expansion and emission, was an effort in this direction. The expansion formula employed by the Author took no account of friction, as in the case of the formula in the Mémorial. The latter showed that friction exercised little influence in the duration of the oscillations of the pencil, and acted for the most part on their amplitude, which was confirmed by experience. In respect of movement of the paper, his own memoir recommended the employment of rigid transmissions to actuate the indicator drum. It instanced the lengthening of the curves resulting when the pace was rapid, and a cord was employed to actuate the paper-drum. The modes used to remedy this evil were demonstrated. The Author carried his analysis further, and showed that the inertia of the drum and the difference of tension of the spring lengthened the curves proportionately in all their parts; while the friction of the drum occasioned a lengthening of the cord, which deformed the curve and might occasion a considerable error in the estimated power. In addition to his other labours, Professor Berndt had studied the lengthening of the cord of indicators, and had used, to control the position of the pencil, an apparatus based on the employment of electricity, and resembling that of Mr. Brightmore. As the result of experiment, Professor Berndt estimated the influence of this cause of error at less than 1 per cent. for admissions varying from 0·3 to 0·5, and lengths of string included between 4 and 7 feet. It was only with very damp cords that he encountered an error amounting to from 1·8 per cent. to 3·6 per cent. He showed also that there was always a minus error, insomuch that the indications of the diagram were always too little. The Author, relying on Mr. Brightmore's experiments, attributed much greater importance to the error occasioned by the lengthening of the cord, and estimated it at from 1 per cent. for the Owens College engine, to 8½ per cent. for a diagram from a locomotive, given by Mr. Porter; and he added that it might attain 20 per cent. for a condensing engine with a cut-off of one-tenth. This was an important fact, for drawing attention to which great thanks were due to Professor Reynolds. If it were considered that the inertia of the drum might deform the indicator-curves for engines with short connecting-rods, and of which the rotary movement was not uniform, such as was the case with marine-engines; that further, according to Mr. Brightmore, the lengthening of the string was not well-defined and did not always obtain in the same manner, then the conclusion seemed to be that it was prudent, in order to be beyond the possibility of this kind of error, to actuate the paper by rigid transmission, allowing of as little play as possible in the articulations. Instruments such as the Cody indicator, in which the paper had a uniform motion, furnished also a solution of the problem in the case of engines whose motion of rotation was uniform. Mr. de Maupeou.

Mr. Richards. Mr. C. B. RICHARDS, in expressing his appreciation of the Papers by Professor Reynolds and Mr. Brightmore, thought it very probable, if not certain, that the best of the most modern forms of indicators, when applied in the careful manner now generally practised by educated engineers, did give indications which were nearly correct. He had recently examined critically a large number of indicator-cards, taken from various engines, with modern indicators, used interchangeably at the opposite ends of the engine-cylinders, and he could not discover a difference of more than $\frac{1}{50}$-inch in the lengths of the diagrams from opposite ends, and in most of them there was no perceptible difference, although the proportions of the indicator-attachments were such that the string for driving the indicator-drum at the back-end must have been more than double the length of the string for the front-end. This of course did not prove the absence of defects in the drum's movements; but, taking into consideration also the results obtained by Mr. G. H. Barrus, with the ingenious device contrived by Mr. Gowing for testing the effect of drum-friction and elasticity of the drum-string, it seemed to him that evidence was afforded of the practical correctness of the movements of the drum in the best indicators. This was when the drums were driven by the comparatively inelastic and short drum-string which, in the United States at least, was generally employed by engineers who understood how to use the indicator. The errors of the movements of the pencil were not so easily ascertained; but Mr. Barrus had computed the moments of inertia for three of the most modern forms of the parallel-motion indicators. Having compared these with the weights and inertia of the corresponding parts of the Richards indicator in its older form, and having made use of the formula deduced by Professor Reynolds for the effects of the inertia upon the diagram, Mr. Richards concluded that cards practically free from serious errors, arising from this cause also, could be obtained from these more modern indicators.

Mr. Rigg. Mr. ARTHUR RIGG observed that the Richards indicator was undoubtedly a great improvement on its predecessors; but as speeds had increased, it had long ago been distanced by others, and nobody would think of using it for very accurate researches. Modern indicators had been mostly of American manufacture, and

Mr. Rigg. none had excelled the Crosby indicator. In every indicator there arose sooner or later a rate of piston-movement, where the reciprocation of the piston and its attachments exercised too detrimental an influence on the diagrams; but the object of any inquiry was surely to ascertain the practical limits of the usefulness of the diagrams. When a mathematician complained of want of absolute accuracy on the part of engineers, he should take care that his own calculations and his own language were free from a like reproach. Indeed, a condition had been laid down in the Paper that "the pencil of an indicator shall under every change of pressure instantly move. . . ." But an "instantaneous" movement from a state of rest was impossible; and, when dealing with a subject in a supposed accurate manner, it would have been as well if Professor Reynolds had used terms that were exact: moreover, were such a movement possible, it was not in the present case required. Entry of steam into a cylinder absorbed a certain pressure, imparting velocity to its volume in transmission through ports and passages; it also occupied a definite time, dependent generally upon the speed of piston and ratio of port-area to cylinder-area. All, therefore, an indicator-piston need do was to follow this movement in a corresponding proportion, and with a minimum absorption of steam-pressure in so doing. Thus it was not so much the speed of an engine, as the rapidity with which pressure increased in a cylinder, that limited the suitable weights of an indicator-piston and its attachments; and hence diagram-errors, unimportant at slow speeds, became accentuated and enlarged where the admission of steam was rapid, as in quick-running engines. It might be pointed out, in regard to errors in the drum-spring and cord neutralizing one another, that for a certain angular movement a longer spring, like that used in the Crosby indicator, gave less variation in pull than the shorter spring of the Richards and other indicators. This supposed evil regarding barrel-springs could therefore be made into a positive benefit, and Professor Reynolds' greater difficulty became non-existent; but the lesser difficulty relating to the influence of connecting-rod length, which he dismissed as of no consequence, was really the greater. The effect of a connecting-rod was to increase the cylinder-capacity at the "lower," or further end from the crank, and to diminish it at the front, or near end, in corresponding times, and as this must have some influence upon indicator-diagrams, he thought it would have been better if Professor Reynolds had supplied some information on the subject, rather than to have dismissed it as unworthy of notice. Taking a ratio of connecting-rod to crank-radius as, for example, $\frac{1}{4}$, it followed that at one end $\frac{1}{4}$ more than the mean and at the other end $\frac{1}{4}$ less than the mean pressure would be needed to start the reciprocating parts of the engine, and a corresponding effect occurred with the paper barrel of the indicator when used upon an engine possessing such proportions. Mr. Rigg.

It was not to be supposed that absolute perfection could be reached by an indicator-diagram, but it was the true interpretation that was of surpassing value; and he considered Professor Reynolds had failed to give this. It might be interesting to supply an explanation of the effects which produced the falling of pressure correctly indicated by that diagram. After the closure of exhaust, compression began in the cylinder by the piston approaching the cylinder-cover, and the temperature and pressure of the exhaust rose simultaneously. This action went on until the piston, always moving more and more slowly, got near enough to the cover to exercise a cooling influence upon the confined exhaust, that exactly balanced the heating influence due to compression. Here the pressure remained stationary. Then the cooling influence became greater than such heating as might be due to the slow compression in progress, and the temperature fell, with a corresponding fall of pressure, until admission of fresh steam caused the pressure-line to rise again.

Professor Smith.

Professor Robert H. Smith, of Mason College, Birmingham, observed that the errors pointed out in the action of Richards indicators had long been known to exist, but he was not aware of any previous attempt to quantify them. On account of these evident failings in the older indicator he had discarded it several years ago in favour of Darke's high-speed indicator. The students at Mason College had used these indicators constantly for the last four years. Although far from perfect, they were, he thought, much superior to the older and heavier instrument. They had been used up to about 230 double reciprocations per minute, without oscillations appearing in the curve on any occasion due to the fault of the instrument. No isochronous oscillations had, in fact, been observed in the curves drawn.

The following were the weights of the moving parts of this instrument:—

	Oz.	Oz.	Oz.	
Weights of springs	$\frac{1}{4}$	$\frac{3}{8}$	$\frac{7}{16}$	
	Inch.	Inch.	Inch.	
Scale of springs	$\frac{1}{16}$	$\frac{1}{36}$	$\frac{1}{96}$	= 1 lb. per square inch.
			Oz.	
Weight of piston and rod without cross-head .			$\frac{3}{4}$	
Weight of cross-head, lever, and pencil cross-head			$\frac{3}{8}$	

Weight of total moving parts from $1\frac{3}{8}$ to $1\frac{9}{16}$ oz., according to the spring used.

Professor Smith.

Subtracting from this last half the weight of the spring, the result was that the equivalent total moving weight was no more than from 1¼ to 1⅜ oz. The ratio of multiplication from piston to pencil was the same as in the old Richards indicator, namely 4. The maximum vertical stroke of the piston was ⅜ inch, and that of the pencil 1½ inch. The length of the lever was only 2 inches, and the weight of the lever and of the pencil cross-head had the centre of gravity very nearly directly over the piston-rod centre. The diameter of the piston was the same as in the old Richards indicator.

He had measured the friction at the various rubbing-surfaces when the instrument was cold. The friction of the piston and the rod without the lever attached was ¼ oz. each way. The friction of the whole mechanism mounted in working order, but without the pencil touching the paper, was ¼ oz. each way at the middle and at the top of the stroke, but ½ oz. each way near the bottom of the stroke. Thus the extra friction of the lever-joints and of the pencil cross-head sliding in the vertical slot seemed to be inappreciable at mid-stroke. These forces were all measured at the piston-rod head, as were also the following, which gave the frictional resistance of the whole mechanism plus that of the pencil-marking on the paper. During these measurements of friction no spring was inserted. If the thrust of the spring were not perfectly axial, of course the actual friction would be somewhat greater than that stated:—

Friction measured at . . . . . . Piston-rod head	Pencil marking on paper.
1½ oz. each way	Faint line.
3 ,, ,, ...	Distinct (good working) line.
6 ,, ,, ...	Moderately heavy ,,
8½ ,, ,, ...	Heavy ,,

Thus in this instrument the pencil-friction was far more serious than any other item of resistance hindering the free motion of the pencil. The last force, namely ½ lb., would involve an error of 1 lb. per square inch in the indication as the piston-area was ½ square inch. The forces necessary to move the paper barrel either way were measured by pulling the cord by a small Salter's spring-balance reading to half ounces. The cord-tensions, when the pencil stood ¼ inch from either stop, were:—

	Lbs.	Ozs.		Lbs.	Ozs.
Starting outstroke . .	3	6½	Ending outstroke . . .	6	7¼
Ending instroke . .	3	2½	Starting instroke . . .	5	14½
Friction = ½ Difference =		2	Ditto		= $4\frac{3}{16}$

Professor Smith.

Thus, without the pencil touching the paper, the friction at the barrel-pivot, as measured by the change of cord-tension it produced, varied from 2 ozs. at the inner end to 4¼ ozs. at the outer end of the stroke. His measurements of the change in cord-tension caused by pencil-friction were not very satisfactory, but none of the measurements fell outside the limits of ½ oz. and 1 oz. In comparing this with the above statement of frictional resistance to vertical motion of the pencil, it must be remembered that the cord-tension acted with the same leverage nearly as the longitudinal pencil-friction, whereas the vertical pencil-friction had to be multiplied by 4 to reduce it to the balancing force at the piston-rod. All these frictional forces were measured at a low rubbing velocity, but not zero velocity. The following tests of indicator-cords and wires might be interesting. A new indicator-cord stretched 1⅛ inch with a pull of 40 lbs., and broke with one of 49 lbs. The same piece re-tested stretched thus:—

	Inch.	Inch.	Inch.	
In 10 inches new stretch	$\frac{9}{32}$	$\frac{3}{8}$	$\frac{1}{2}$	broke, leaving ¼ inch permanent stretch.
	Lbs.	Lbs.	Lbs.	Lbs.
Pull	30	40	50	51

The same piece tested a third time gave—

	Inch.	Inch.	Inch.	Inch.	
In 10 inches new stretch	$\frac{1}{4}$	$\frac{5}{16}$	$\frac{3}{8}$	$\frac{9}{16}$	broke.
	Lbs.	Lbs.	Lbs.	Lbs.	Lbs.
Pull	30	40	50	55	57

During this third test the cord, therefore, stretched at the rate of 0·00075 of its own length per lb. of tension.

An old indicator-cord that had been in use for a long time gave the following results:—

Tension in lbs.	30	40	50	60	70	80
Stretch in inches	0·30	0·35	0·40	0·47	0·50	0·54
Permanent stretch	..	0·10	0·12	0·14	0·17	0·18

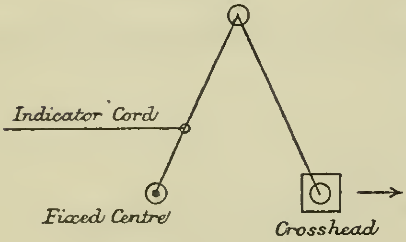
Tension in lbs.	90	100	110	112
Stretch in inches	0·58	0·61	0·66	..
Permanent stretch	0·25	0·28	0·30	broke.

Excluding the permanent stretch, this cord stretched 0·0005 of its own length per lb. of tension.

Taking 50 inches of cord, and 5 ozs. as the frictional force changing its length, the paper would be displaced from its true position no more than 0·008 inch. This would have no appreciable effect on the area of the diagram. A test of steel piano-wire, Professor Smith. 0·014 inch in diameter, gave a stretch of 0·0001 of its own length per lb. of tension up to about 30 lbs., and about double that at higher forces, breaking under a pull of 58 lbs. This wire seemed to be very suitable for indicating, provided that it was not led over small guide-pulleys; but the rapid to-and-fro bending and unbending over small pulleys snapped it very quickly.

In saying that the temperature of the spring never rose above 212° Fahrenheit, Mr. Brightmore seemed to have forgotten that heat was conducted to the spring through the metal from the piston, whose under-surface came in contact with high-pressure steam, and therefore became very hot. The rude mechanism (Fig. 53) only yielded good results provided the angle to which the lever was thrown to one side of its central position was small. If this angle were not small and the stroke considerable, the vertical motion of the cord end would be greater than should be allowed. To make this angle small, when the stroke was long, a very long lever must be used. The slot was then high above the engine, and the cord required to be led over pulleys, and became unnecessarily long, leading to error by its stretching. He thought the leading over pulleys must greatly aggravate the errors due to stretching. The gear which he used on the Mason College engine, and elsewhere whenever practicable, consisted of two levers of equal lengths. These were pinned together, and the end of one was pinned to the crosshead, while the other end of the second lever was pinned to the engine-frame, in line with the stroke of the cross-head pin. The motion of the joint connecting the two levers had its component parallel to the stroke, exactly half that of the piston, while that of any point in the lever with the fixed centre was always exactly in the same proportion to the piston motion. The cord end had never any transverse travel more than about one-third of the paper stroke. This, of course, could introduce no measureable error. The cross-head stroke must not pass, nor indeed come very near, the fixed centre of the cord-lever. He had for some years been considering the design of an indicator in which the various errors appearing in the instruments in present use should be avoided as far as practicable, but he had not yet discovered any method of minimizing the pencil-friction applicable to a piece of apparatus to be used under the somewhat rough conditions of indicator practice.

FIG. 53.



Mr. Stroudley.

Mr. W. Stroudley drew attention to the errors that occurred by reason of expansion of the paper on which the figure was delineated by the indicator. This amount might vary from 1 to 2 per cent. of the total power shown, the variation depending upon the thickness and nature of the paper. Should the paper be exposed to damp or steam, as was likely in a marine-engine room, or to moist atmosphere on a locomotive, and the figure be afterwards measured as was usual, in a dry office, the apparent indicated power would be considerably less than the actual. He found he could obtain a more exact figure by fitting the indicator to the end of a short conical tube, bent to such a form as to place the indicator in an upright position. The taper of the tube was from $\frac{3}{4}$ inch in internal diameter at the end next the cylinder to $\frac{1}{2}$-inch diameter at the end where the indicator was attached; the total length of the tube was about 6 inches. He also found that the gear for working the indicator must be so fixed that the cord used should be reduced to a total length of less than 1 foot. He had adopted steel wire for working the indicator on the Brighton Company's marine-engines.

Mr. Willans.

Mr. P. W. Willans thought that it would have very much added to the value of Professor Reynolds' investigations, if he had not confined himself practically to a single indicator in the course of his experiments. If there was a reasonable chance that hitherto the nominal indicated power of engines had been 10 to 20 per cent. too small, or even 3 per cent. too small, it was a very serious matter; but as a practical engineer, he must protest that there was no necessity to measure the diagrams to $\frac{1}{10,000}$ inch, or to correct all the readings by such equations as (2) page 5. He considered that the first four principal causes of disturbances in the diagram had been generally understood, although the tendency seemed very rightly to have taken the line of eliminating, as far as possible, these disturbances, by suitably designing the indicator, rather than that of correcting them by elaborate formulas. Taking these disturbing causes in the order in which Professor Reynolds had alluded to them: (1) The effect of the inertia of the piston of the indicator and its attached weights had been quite sufficiently understood to induce many people to try to lighten them, and they had succeeded in improving on the Richards indicator in a greater or less degree. For instance, although Mr. Brightmore mentioned that the rod D F in the Richards indicator had as much effect in causing oscillations as all the other moving parts together, he did not allude to the fact that in the Crosby indicator, for instance, not only was this rod, but

Mr. Willans. also the rod F H absent. He had used these Crosby indicators since the first came over to England. He was much struck by the care which had evidently been taken in its design, and although he had tried many indicators at speeds of 300 to 500 revolutions per minute, he had not found any others so reliable. With respect to the friction of the pencil on the paper; this was a point which he did not think introduced any appreciable error in good hands, but in careless or unaccustomed hands it might be a disturbing cause. He was sorry to have to confess, that in this respect, so far as he knew, the American instrument was ahead of any made in this country, because it provided a screw-adjustment by which the pencil was brought as gently as was desired to the paper. He had not seen this on any English instrument; but Fnglish makers would do well to note it, for it not only provided a means for taking diagrams as accurately as possible in this respect, but its presence warned inexperienced persons that care was necessary. The effect of the varying tension of the spring and of the inertia of the drum, and the manner in which they eliminated one another at certain speeds had, he fancied, been long pretty well known to practical men; as also the necessity for a strong spring. When he used Richards indicator for high-speed diagrams, he always helped the barrel-spring by a strong india-rubber spring outside. The quickly increasing tension of the rubber spring was just what was wanted for destroying the effect of the inertia of the barrel, and caused the pull on the cord to be more nearly constant throughout the stroke. In this respect also he noticed that the makers of the Crosby indicator had shown great knowledge of the subject, as they had provided not only a light drum and a shorter and a stiffer barrel-spring, but they had also provided for very easy adjustment of the latter. Now with respect to the friction of the drum-mechanism, he thought that the instrument used by Professor Reynolds must have had a very light barrel-spring, by which he meant a small increase in tension per inch in proportion to the length of the cord. He knew by experience that where a long cord was used, with the older Richards indicator at any rate, the barrel-spring always required a little assistance from without, but probably the main difficulty in the matter was the one which was met when in Professor Reynolds' own words, "The indicator was then taken to pieces, cleaned, and oiled." The fact that such an obvious precaution could be overlooked in such a case, for so long, did not tend to give him much confidence in the practical parts of Professor Reynolds' Paper, although in many of his theoretical conclusions he was probably

Mr. Willans.

right. There were several other chances of error in an indicator to which he fancied Professor Reynolds did not allude. One was slackness in the joints, which usually increased the area of a high-speed diagram; another was the friction of the piston in its cylinder. The observer could not keep tapping an indicator with a hammer when taking diagrams; the most practical way out of the difficulty was to let the piston be loose enough to leak slightly, rather than tight enough to stick, always supposing that the indicator was close to the cylinder, and the pipes of fair size. There was one important omission in Professor Reynolds' criticisms of the various diagrams which he illustrated. Mr. Willans did not see that in any case (as for instance Fig. 11, in which he assumed that the length of a diagram was wrong) mention was made of the stroke of the mechanism from which the indicator-barrel was actuated. The fact of the diagram being 10 per cent. shorter than the stroke of the mechanism would have been a conclusive argument, and better than any deductions drawn from expansion-curves. Some diagrams had been sent to him recently, taken by a Crosby indicator from an engine in which the revolutions were varied from 120 to 370 per minute, and in which the steam-pressure in the cylinder remained practically the same. He thought that these diagrams might present variations in length, but he found that between the longest and shortest the difference was not much more than 1 per cent. These were taken without any special precautions, and the barrel-spring was not so strong as the one used by himself for high speeds.

Professor Reynolds.

Professor REYNOLDS in reply to the correspondence, said he fully appreciated the weight of such an opinion as that of Mr. Richards respecting the accuracy of the results of modern indicators. Although his own opinion coincided with that of Mr. Richards, yet in matters of definite measurement opinion had no place, and to secure confidence in any system of measurement it was necessary to know the limits of its accuracy. The modern forms of the indicator, which were not in anything like general use two years ago, when the investigation described in Mr. Brightmore's Paper was made, were doubtless capable of indicating at higher speeds, according to the theory given in the Paper. But as regarded the main sources of error which arose from friction of the pencil and the drum (where cord was used), they were at a disadvantage, inasmuch as they had smaller pistons, greater exaggeration of the motion of the pencil, and shorter length given to the diagram to deal with. That these disadvantages where they existed might be compensated by other improvements was possible, but, as

Professor Reynolds. had been shown of the Richards indicator, general consistency in results was not a proof of freedom from error; and, in however high an opinion these instruments might be held, it would be only an opinion until the limits to their accuracy had been determined in some such manner as that adopted in Mr. Brightmore's experiments. In reply to the complaint that no mention had been made of the expansion of the piston of the indicator at different temperatures, which Mr. Isherwood considered the chief source of inaccuracy in the indicator, it was stated that a careful estimate of this error showed the Author that the extreme variation of the area of the piston, for a variation of temperature of 100° Fahrenheit would only be 0·5 per cent.; and as regarded Mr. Isherwood's and Professor Smith's remarks about the temperature of the springs being greater than 212°, it was stated that within the Author's experience if the indicator was kept too long in connection with the steam cylinder, trouble was found from the water condensed above the piston. This showed that when the indications were taken the temperature was below, not above, 212° Fahrenheit. Respecting Professor Smith's experiments as to the elasticity of cords, it had been pointed out that, as with all twisted or woven cords, the coefficient of elasticity diminished very rapidly as the load increased; cords stretched nearly as the square root of the load; the elasticities determined with loads of 30, 40, and 90 lbs., afforded no test as to what it was with 3 or 4 lbs., which was the tension in Professor Smith's indicator. As a general rule cord would be four or five times as elastic when loaded to one-thirtieth its breaking load as when loaded to one-half, though of course this depended on how far the cord had not been sized. More than this, there was no reason to discard the residual set, or, as Professor Smith termed it, permanent set.

Taking this into account at a load of 3 lbs., Professor Smith would in all probability find that the old cord in its condition of use would show a coefficiency of ability to stretch of 0·005 in his calculation for a 50-inch cord, and frictional force of 5 ozs., would give a stretching of 0·08 inch. This was as much as could be found by measurement with Mr. Brightmore's indicator, when the motion was reversed slowly, although not half as much as was shown by the electrical pricking at speeds of 127 revolutions at the ends of the diagrams. At the time of writing the Paper it was assumed that this difference was due to an increase of friction, particularly that of the pencil, with the speed—a view which was apparently borne out by the difference being somewhat greater at the higher speeds. But on subsequent consideration it appeared

Professor Reynolds. that the difference was not all due to increase of friction, but to some extent to the residual set in the cord caused by the excessive tension at the back-end, or slackness at the front-end, necessary to start the drum to overtake the engine-piston after the former had been resting at the ends of its stroke to allow of the tension of the cord adjusting itself to the reversal of the friction. With a cord or wire liable to stretch there would be such inequality in the tension, but with a perfectly elastic cord or wire the effect would be a vibratory motion imparted to the drum, which was what appeared in Figs. 27, 28, 31 and 32 with wire. With a cord susceptible of residual set although there would be a vibratory motion, due to the elastic stretching, there would also be a residual set which would last throughout the stroke, causing the drum to lag behind the position due to the acting friction. This was also shown in Figs. 29, 30, 33, and 34, taken with string, in which in either stroke the first interval was short, the second long, and the third, short with the lower speed long with the higher, and so on. The above effect would doubtless increase with the speed, as it seemed to do in the diagrams; but in other respects its recognition did not appear to alter in any important particular the conclusions drawn in the Paper.

Lastly, errors arising from faulty and long steam-connections should doubtless be considered in a discussion of the errors of the indicator, and their not having been mentioned in the Papers, was an omission; such, however, was not uncommon where a precaution had become the rule. The Authors were obliged to Mr. Rich and to Messrs. Donkin and Salter for having directed attention to this omission.

ADDENDUM TO MR. BRIGHTMORE'S PAPER.

JANUARY 9TH, 1886.

Mr. Brightmore. In the discussion, Mr. Mair and Professor Kennedy referred to very considerable differences in the position of the piston of the indicator for equal loads during its ascent and descent, apparently due to some kind of friction. As any such differences that there might have been in the experiments recorded in the Paper, made to determine the correctness of the spring, had been purposely eliminated by tapping, the following experiments were carried out with the same indicator, the same springs, and in the same way as before, the readings being taken at 212° Fahrenheit. The weights were in each case put on and taken off in the gentlest possible manner. So far as they went these experiments served to show that the difference between the readings (with the

Mr. Brightmore.

same weights on) taken on adding and subtracting weights was nearly constant (increasing slightly), and with the highest loads was less than 5 per cent. These results therefore agreed with those of Professor Kennedy as to the importance of this friction, which might increase the area of the diagrams by as much as 5 per cent., although further investigation as to this point was required.

EXPERIMENTS ON JANUARY 2ND, 1886, AT OWENS COLLEGE.

20-SPRING at 212° FAHRENHEIT (WITHOUT TAPPING).

Adding Weights.				Subtracting Weights.			
Weight.	Reading.	Difference.	Difference per lb.	Weight.	Reading.	Difference.	Difference per lb.
lbs.	Inch.	Inch.	Inch.	lbs.	Inch.	Inch.	Inch.
C	0·6891			28	1·2671		
		0·0236	0·0236			0·2064	0·0147
1	0·7127			14	1·0607		
		0·0508	0·0254			0·1847	0·0264
3	0·7635			7	0·8760		
		0·1023	0·0256			0·1035	0·0259
7	0·8658			3	0·7725		
		0·1774	0·0253			0·0514	0·0257
14	1·0432			1	0·7211		
		0·2239	0·0159			0·0249	0·0249
28[1]	1·2671			C	0·6962		

20-SPRING at 212° FAHRENHEIT (TAPPING).

Adding Weights.				Subtracting Weights.			
Weight.	Reading.	Difference.	Difference per lb.	Weight.	Reading.	Difference.	Difference per lb.
lbs.	Inch.	Inch.	Inch.	lbs.	Inch.	Inch.	Inch.
C	0·6962			28	1·2669		
		0·0239	0·0239			0·2091	0·0149
1	0·7201			14	1·0578		
		0·0500	0·0250			0·1807	0·0258
3	0·7701			7	0·8771		
		0·1020	0·0255			0·1034	0·0258
7	0·8721			3	0·7737		
		0·1800	0·0257			0·0514	0·0257
14	1·0521			1	0·7223		
		0·2148	0·0153			0·0257	0·0257
28	1·2669			C	0·6966		

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[1] 28 lbs., *i.e.*, a pressure of 56 lbs. overweighs the 20-spring, the spirals coming in contact with each other.

Mr. Brightmore.

32-SPRING at 212° FAHRENHEIT (WITHOUT TAPPING).

Adding Weights.				Subtracting Weights.			
Weight.	Reading.	Difference.	Difference per lb.	Weight.	Reading.	Difference.	Difference per lb.
lbs.	Inch.	Inch.	Inch.	lbs.	Inch.	Inch.	Inch.
C	0·6489			28	1·0927		
		0·0163	0·0163			0·2164	0·0155
1	0·6652			14	0·8763		
		0·0308	0·0154			0·1103	0·0158
3	0·6960			7	0·7660		
		0·0625	0 0156			0·0590	0·0148
7	0·7585			3	0·7070		
		0·1106	0·0159			0·0387	0·0193
14	0·8691			1	0·6683		
		0·2236	0·0160			0·0155	0·0155
28	1·0927			C	0·6528		

32-SPRING at 212° FAHRENHEIT (TAPPING).

Adding Weights.				Subtracting Weights.			
Weight.	Reading.	Difference.	Difference per lb.	Weight.	Reading.	Difference.	Difference per lb.
lbs.	Inch.	Inch.	Inch.	lbs.	Inch.	Inch.	Inch.
C	0·6512			28	1·0973		
		0·0158	0·0158			0·2172	0·0155
1	0·6670			14	0·8801		
		0·0316	0·0158			0·1133	0·0162
3	0·6986			7	0·7668		
		0·0649	0·0162			0·0654	0·0163
7	0·7635			3	0·7014		
		0·1107	0·0158			0·0317	0·0158
14	0·8742			1	0·6697		
		0·2231	0·0159			0 0173	0·0173
28	1·0973			C	0·6524		

17 November, 1885.

Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

The discussion, upon the Papers upon "The Steam-Engine Indicator" by Professor Osborne Reynolds and Mr. Arthur William Brightmore, occupied the entire evening.

24 November, 1885.
Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

(*Paper No. 2107.*)

"High-Speed Motors."[1]

By JOHN IMRAY, M.A., M. Inst. C.E.

MANY members of The Institution of Civil Engineers may recollect the time when the science of thermo-dynamics was all but unknown, and when the principle of the conservation of energy, if it was accepted by men of science, was certainly not appreciated by the practical engineer.

In those days a steam-pressure of 5 to 7 lbs. above the atmosphere was considered high pressure; and one of the usual and necessary fittings of a steam-boiler was a vacuum-valve, or safety-valve, opening inwards, to prevent collapse of the boiler by excess of external pressure. In those days long connecting-rods were considered essential; it was believed by many that a short connecting-rod involved actual loss of power, not merely on account of friction due to lateral strain, but also on account of mere obliquity in the action of the rod. There was also a prejudice in favour of a very moderate speed of piston, which, it was thought, should not exceed a rate of 220 to 250 feet per minute. As the steam-engine, in its infancy, was chiefly employed for pumping, it was natural to make it with a beam, applying the piston at one end and the pump-bucket at the other. The same form was generally adopted in engines employed for giving rotation to a shaft.

Even in marine-engines a modified form of the beam-engine was universal, the beam being in this case divided into two side-levers, which were mounted at a low level in order to avoid top-hamper, and to communicate motion in a convenient manner to the paddle-shaft overhead. For a long period marine-engineers in America adhered to the early form of engine with its beam oscillating high above the vessels' deck. When the screw-propeller began to be adopted, the slowly moving side-lever engine was for some time employed, and gearing was used to multiply the speed as required for the propeller-shaft.

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[1] The discussion upon this Paper was taken together with that upon the following one.

Meanwhile, in America engineers had been gradually raising working-pressures, at the risk of explosions, which were frequent and serious. The Cornish engineers had also been moving in the same direction, and they almost startled the engineering world by the results which they obtained by the use of high initial pressures and large ranges of expansion.

In the locomotive, where simplicity and compactness are essential, the direct-acting form of engine was at once adopted and worked with considerable steam-pressure; and the invention of the tubular-boiler, furnishing a very large heating-surface in a compact and strong form, enabled engineers to command high pressures without great danger.

For a long time it was considered necessary to balance, or approximately balance, the moving parts; and indeed, when very low pressures were employed, approximate counterpoise of the moving weights was essential. By the use of horizontal-engines, as in the locomotive, this was rendered unnecessary, and this arrangement soon came to be adopted for marine-engines working screw-propellers. In these it was peculiarly applicable, as it kept the weights low, and brought the power to bear directly on the propeller-shaft at the required low level. It was also well adapted to the very short strokes employed to obtain considerable velocity of rotation without material increase of the piston-speed.

So far, in the history of steam-motors, there was comparatively little progress in respect of speed, though there was a great advance in the direction of high pressure and considerable expansion, as well as in the simplification of the mechanism, and in the removal of those racking strains which manifest themselves in the beam-engine. The marine engine-shaft seldom made more than 70 or 80 revolutions per minute, and a locomotive driving-wheel, 6 feet in diameter, had not to make 250 revolutions per minute for a speed of 50 miles an hour.

It was not until centrifugal-machines, torpedoes, and, most of all, dynamo-electric machines, had to be worked, that a demand arose for very highly-speeded motors suited for direct-driving, so as to avoid the noise and wear of toothed-gearing, or the slip and strain of driving-belts. Much has of late been done in the contrivance of highly-speeded prime-movers; but much still remains to be done, and it is worthy of consideration as to how invention should be best directed for providing high-speed motors, such as are now so largely required for many purposes.

At first, some form of rotary-engine would seem to commend itself as being peculiarly applicable where great speed of rotation is required. There is something captivating in the idea of obtaining rotation from the continuous uniform flow of a fluid under pressure. Accordingly many engineers have exercised a great amount of ingenuity in devising various forms of rotary-engines. Among others, the Author yielded to the fascination. When he had succeeded in devising a rotary-engine and getting it to work fairly, his former master, the late Mr. Joshua Field, Past-President Inst. C.E., pointed out to him that natural movements, such as walking, flying, and swimming, are reciprocatory, and that, though it may be convenient to apply power by rotation, as by the revolving shaft, the paddle or screw, it is on the whole better to obtain power by reciprocation, as in the action of the muscles of the animal frame.

Very few engines are purely rotary; most of those that are so called involve intermittence or reciprocation in some of their movements. They usually have slides or flaps that move to and fro, so as to act either as abutments to the working pressure, or as pistons moving through part of a revolution in obedience to that pressure. In some cases these parts have their movements so arranged that one part acts for a time as an abutment, remaining stationary or nearly stationary, while another is propelled by the working fluid. Almost all such engines present great difficulties in their packings, and are subject to considerable inequalities in the wear of their rubbing surfaces.

Among engines of a purely rotary character may be mentioned the reaction-wheel, probably one of the earliest forms of apparatus in which an elastic fluid, such as steam, was used to produce continuous rotation. Where very great speed of rotation and extreme simplicity of structure are demanded without regard to economy of fuel, this form of engine may be serviceable. But, when it is considered that nothing short of infinite velocity of rotation suffices to bring out full efficiency, the reaction-wheel cannot be commended for many practical purposes.

The turbine is another kind of purely rotary-engine. But, in this apparatus, the extremely high velocity of rotation necessary to realize even moderate efficiency from a current of steam, forms an objection that cannot easily be got over. It would probably be possible to obtain considerable reduction of speed by employing steam of great density, and causing it to act on a series of turbine blades, giving out part of its velocity of flow to each blade, or set of blades, in succession. Here again, however, the engineer is met by a very important element of loss, the friction of the fluid on the surfaces of the blades, and of the stationary guides that must be employed to give direction to the operating currents. This view seems to be borne out by the results of experiments made with a species of steam-turbine. The apparatus was constructed somewhat like the liquid brake-dynamometer, invented by the late Mr. W. Froude, M. Inst. C.E., but it was arranged to act in an inverse manner. In Mr. Froude's brake there is a wheel and a casing, both made with what may be termed ratchet-cells, in which liquid is put into rapid vortical movement by the revolution of the wheel, while the casing remains at rest. In the apparatus as modified to form a steam-turbine, jets of high-pressure steam were directed into some of the ratchet-cells, so that the steam moving vortically from cell to cell, gave a very rapid rotary motion to the wheel. With steam at a pressure of 30 lbs. per square inch above the atmospheric pressure, the highest efficiency was obtained when the wheel, which had a diameter of 6 inches, made 12,000 revolutions per minute. The quantity of steam used was ascertained by condensing the exhaust and measuring the water of condensation. It was nearly double the quantity required in an ordinary non-condensing engine to produce the same amount of power, a result by no means encouraging. Greater success is said to have attended the working of a multiple steam-turbine recently invented by the Hon. C. A. Parsons, Assoc. M. Inst. C.E.

In the reaction-wheel and the turbine, the action is due to the conversion of velocity into pressure. In a third form of purely rotary-engine, which acts tolerably well as a pump or blower, but which does not seem to have met with success as a steam-engine, the fluid acts by pressure simply. In this case, a pair of toothed-wheels, geared together, are fitted within a casing. Steam, admitted on one side of the casing, in its effort to pass to the other side, presses on the outer teeth of both wheels, but only on an inner tooth of one of them, as this tooth for a time shrouds a tooth of the other wheel. There is thus an unbalanced pressure on the area of the side of a tooth, causing the wheels to rotate. As the wheels must accurately fit the sides of the casing, and as these teeth must accurately fit each other, and also fit the ends of the casing, there is great difficulty in making or packing such an engine so as to prevent leakage, especially after it has been subject to a little wear. It presents, moreover, the great disadvantage that the steam does not act expansively, but flows through the casing at its full pressure, not expanding until after it has performed its work. Many different forms of gearing have been tried in engines of this kind, but none of them appear to have been successful.

Another rotary-engine, the Dudgeon-engine, also consists of a pair of toothed-wheels geared together; but in this case, the steam is admitted at the ends of the teeth, into the small clearance spaces that successively present themselves to the admission ports. These ports are so situated that the steam does not enter a clearance space until it has just passed the centre line joining the axes of the two wheels. The steam, expanding in this space, acts on the excess of area presented on the side of the more forward of the two teeth, over that presented on the side of the more backward tooth, and so gives propulsion to the wheels. The great difficulty in constructing such an engine, and maintaining it in efficient condition, lies in the accurate shaping of the teeth, for each tooth, although it touches the neighbouring tooth only along a line of contact, has to maintain that contact during engagement so completely as to prevent leakage of steam from the space in which it is expanding.

Almost all other so-called rotary-engines, such as the disk-engine, and those of similar type, involve dead-points, intermittencies, or reciprocations, which take away from their purely rotary character. In some, several cylinders are mounted on a revolving boss, having their pistons acting against an abutment eccentric, or inclined, to the axis around which the cylinders revolve. Such engines are really of a reciprocating kind, differing from ordinary reciprocating-engines only in the particular mechanism employed for converting the to-and-fro movements of their pistons into continuous rotation.

It may be said generally, that no good method has been discovered for utilizing the expansive force of an elastic fluid such as steam for the production of motive power, except by means of mechanism involving intermittence or reciprocation. This almost follows as a physical necessity; it is difficult to conceive how the expansive force of a fluid could be made to give movement to a solid body otherwise than by the dilatation of a cavity into which a charge of the fluid is admitted; this charge, by its effort to occupy an increased space, moving a boundary of the cavity containing it. Thus, in an ordinary steam-engine, the charge of steam admitted to a portion of the capacity of the cylinder, moves the piston enlarging that capacity. In like manner, in almost all known forms of rotary-engines, a rotating blade, or flap, performs the part of a piston, being moved onwards in a circular path by the pressure of the expanding charge. This kind of action necessarily involves intermittence; in one phase of the movement there is admission of the charge and a cut-off; during another phase there is dilatation in an expanding space; and in a third phase there is discharge of the expanded fluid, the space that was occupied by it being again contracted to a condition ready for the reception of a succeeding charge. The alternate expansion and contraction of the space in which the fluid acts may be effected in either of two ways. In that which may be considered the rotary way, it is generally done by giving the space a form eccentric or oblique to the axis of rotation, so that alteration of its capacity is effected in a continuous manner, undergoing enlargement generally during one-half of each revolution, and becoming contracted during the other half-revolution. Or the alteration of space may be effected by reciprocation of a piston as in an ordinary steam-engine, this reciprocating-movement being by means of a crank, or some equivalent mechanism, converted into rotation.

In the class of reciprocating rotary-engines, there is one which has been carefully worked out by the Hons. R. C. and C. A. Parsons. In this engine two pairs of cylinders, having their axes at right-angles to each other, are fixed in a frame which revolves round a trunnion. Their piston-rods, which are jointed to opposite cranks on a shaft eccentric to the trunnion, cause this shaft to revolve at double the angular velocity of the cylinders themselves. This engine has met with considerable success when worked at high speed.

The subject of rotary-engines cannot be dismissed without reference to two ingenious forms of comparatively recent date.

In one of these, invented by Mr. Tower,[1] two revolving shafts are inclined to one another, and connected by a universal-joint. The gimbal-ring of the joint is filled up so as to form a complete disk or circular diaphragm, and the arms by which the shafts are jointed to this diaphragm are also filled up so as to form two semicircular blades. The central disk and the two blades are enclosed within a spherical shell, in which they can revolve. They divide the interior of the shell into four separate cavities, each of which, during one-half revolution of the shafts, expands from a minimum to a maximum of capacity, and during one-half revolution again becomes contracted to a minimum, thus operating as the equivalent of a cylinder and piston. By the revolution of ports opening into these four spaces, round stationary ports in the bosses of the casing, the admission and emission of steam are alternated. The difficulties of packing and lubricating the working parts are said to have been fairly overcome by the makers of these engines, which give good results in respect of efficiency. The four spaces within the shell act as four single-acting cylinders with pistons would act, one pair of them working a crank at right-angles to the crank worked by the other pair. Thus there is great evenness of movement, and within a small compass there is capacity for a considerable amount of power.

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[1] Institution of Mechanical Engineers. Proceedings, 1885, p. 96.

In the other engine to which reference is now made, the invention of Mr. Fielding, there are also two revolving shafts inclined to one another, and connected by a universal-joint. The gimbal-ring of this joint carries on each side of it a pair of pistons, which work in cavities that are not properly cylinders, but are segments of bent pipe, each being, as it were, a cylinder formed round an axis, which, instead of being straight, is a circular arc struck from the intersection of the axes of the two shafts as a centre. Each of the four pistons moves to-and-fro in this circular path. Here there are the equivalents of four single-acting cylinders and pistons, acting on cranks at right-angles to each other, and so producing evenness of action. The packing in this engine has the merit of simplicity, consisting of ordinary spring-rings held in grooves in the pistons. Both the Tower and the Fielding engines, owing to their compactness and the even character of their action, seem well adapted to operate as high-speed motors for driving electric-machines and for suchlike work.

After this necessarily brief review of several of the principal types of rotary-engines, it may not be unprofitable to consider in some detail, certain conditions inherent to reciprocating-engines, which at first would seem to affect them unfavourably, especially when worked at high speeds.

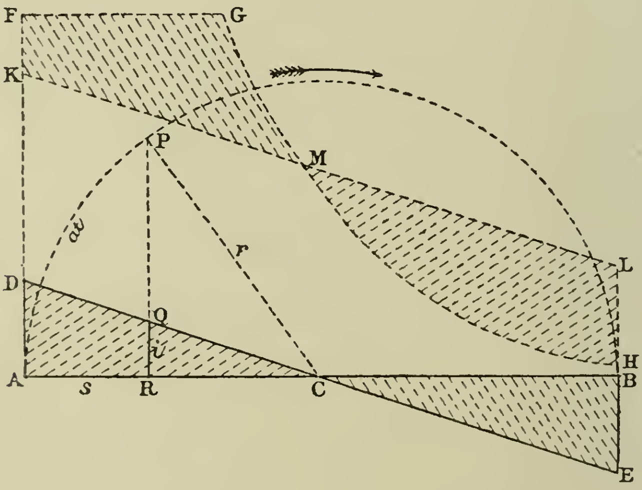
One rather formidable objection to a reciprocating-engine, when it is driven at great velocity, consists in the shock and noise resulting from the change, twice in every revolution, of the strain on the working joints. These joints, by which the connecting-rod is linked to the piston and to the crank-pin, and also the bearings of the crank-shaft, cannot be absolute fits, but must have a certain amount of slackness, and the sudden change at each half-revolution from a pull to a push, and from a push to a pull, causes a blow which is both disagreeable and detrimental. Mr. Brotherhood was one of the first to direct particular attention to this, and to provide against the evil in his three-cylinder engine by making each cylinder single-acting. The joints of each connecting-rod are thus always subjected to strain in only one direction, that of thrust. In the forward-stroke of the piston it is pressed by the steam, and in the backward-stroke it is forced back by the crank in opposition to its friction, and to such back-pressure as the exhaust may exert upon it. These engines have succeeded well as high-speed motors, and have been extensively used for driving the propellers of torpedoes as compressed-air engines, and for driving electric-machines as steam-engines. Of late, Mr. Brotherhood and other makers have been constructing three-cylinder engines with duplicate cylinders to act as compound-engines.

Another condition inherent to reciprocating-engines, which would appear to affect them unfavourably, might under proper adjustment be made to contribute largely to that evenness of action which is especially desirable in high-speed motors. This condition has been touched upon in some treatises, but it has not received that degree of attention which its importance demands. It is therefore proposed to consider it in some detail, as its discussion will probably suggest to engineers a line of thought that may be useful, especially when they are engaged in designing engines suited for high speeds.

In a reciprocating-engine, a certain mass of matter, consisting of the piston and its rod, the cross-head or slipper, and the connecting-rod, has to be put in motion at the beginning of every stroke; it has next to be accelerated until nearly the middle of the stroke, and it has then to be retarded until it is finally brought to rest at the end of the stroke. On the principle of the conservation of energy, this alternate acceleration and retardation can neither add to, nor subtract from, the total power developed during the stroke. The effect, however, at different parts of the stroke is of a very important character, especially when high initial pressure and a large range of expansion are adopted. At first, when the steam-pressure is high, a certain amount of its propelling effect is taken up in mere acceleration of the reciprocating-mass. At last, when the steam-pressure is greatly reduced by expansion, its propelling effect is largely added to by retardation of the reciprocating-mass. Thus the inertia of this mass operates as a very effective fly-wheel or regulator, subtracting from the excess of power at one part of the stroke, and adding to the defect of power at another part of the stroke. An analytical investigation of this action is given in an Appendix to the Paper. Here it may suffice to indicate results with reference to a few diagrams which show these in graphic form.

In order to simplify the inquiry, the case will first be considered of a connecting-rod of infinite length always moving parallel to itself, or, what amounts to the same thing, where the crank-pin is engaged in a slot of a cross-head perpendicular to the line of the stroke. In Fig. 1, C P being the position of the crank when the piston has made the part A R of its stroke, let an ordinate Q R be taken to represent the pressure per square inch of the piston-area, which is then required to effect acceleration of the reciprocating-mass. It will be found that the summits of all ordinates such as R Q are in a straight line D C E, passing through the crank-centre C, and inclined to the line of stroke A B at an angle depending on the weight and velocity of the reciprocating-mass, and the length of the crank-radius C P. The accelerating pressure A D is greatest at the beginning of the stroke; it gradually diminishes until the middle of the stroke, when the piston has attained its maximum velocity, at which point the pressure becomes nil. The retarding pressure then gradually increases until it attains its maximum B E at the end of the stroke. All pressures acting from A to C may be considered positive, the ordinates representing them being drawn above the centre-line to indicate positive inertia or accelerating force absorbed. All pressures from C to B may be considered negative, the ordinates representing them being drawn below the centre-line to indicate negative inertia, or retarding force given back. The area of the triangle D A C above the centre-line represents the amount of work done by the engine in effecting the acceleration of the reciprocating-mass. The area of the triangle E B C below the centre-line represents the amount of work given back to the engine by the retardation of the recipro-

Fig. 1.

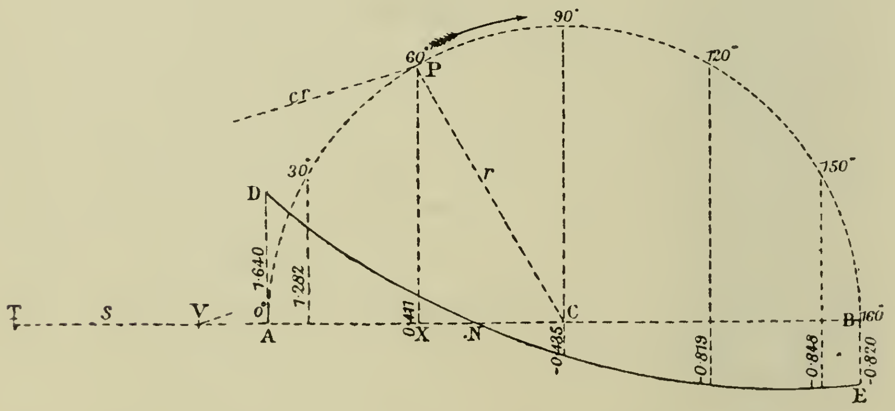


cating-mass. These two areas are obviously equal, that is to say, there is no addition to nor subtraction from the total of the engine-work throughout the stroke caused by reciprocation; but there is a very sensible absorption of work during the first half of the stroke, and an equivalent return during the last half of the stroke. The effect of this absorption and return of work, in assisting to give evenness of action to the engine, may be best seen by drawing on the base-line A B the outline of a theoretical indicator-diagram F G H, showing a cut-off at G, and expansion from G to H. It is then observed that the virtual base is not the horizontal line A B, but the inclined line D E. At the beginning of the stroke, although A F represents the actual pressure on the piston, yet, since A D is that part of it which is absorbed in accelerating the reciprocating-mass, the difference D F is the pressure effective for external work. Again, at the end of the stroke, where the steam-pressure is only B H, since an additional pressure B E is given out in retarding the reciprocating-mass, the effective pressure for external work is E H, the sum of the two ordinates. And, in like manner, throughout the stroke, the pressure available for doing the external work of the engine is measured at any point by the height of the indicator-ordinate above the virtual base-line D E.

A line K M L may be drawn parallel to D E, and averaging the indicator-area. The area of the inclined parallelogram D K L E, which is the same as the figure D F G H E, or as the indicator-figure A F G H B, represents the total external work; but the parallelogram D K L E represents its distribution throughout the stroke when the inertia of the reciprocating-parts is taken into account. Obviously the inequalities that are left for the fly-wheel to subdue are the parts immediately above and below the inclined line K M L. The area K F G M above the line is a measure of the amount of impulse given to the fly-wheel by excess of pressure over that which is required for the engine and its work. The equal area H M L below the line K L is a measure of the amount of impulse received back from the fly-wheel, to make up for defect of effective pressure available for moving the engine and performing its work.

When there is a connecting-rod of finite length, such, for instance, as three times the crank-radius, the inertia-diagram is no longer bounded by a straight line such as D E in Fig. 1; it is a curve of a somewhat complex character, as to which the Appendix is referred to. Fig. 2 illustrates the form of the diagram when the connecting-rod is thrice the length of the crank. In this case the line, instead of passing through the centre C, is a curve cutting the base-line at a point N corresponding with that part of the stroke where the velocity of the piston is at its maximum, the connecting-rod being then nearly tangential to the crank-circle. The ordinates are figured corresponding to every 30° of the half-revolution of the crank, in terms of lbs. per square inch, it being assumed that the crank-radius is 1 foot, that the reciprocating-parts have a weight equivalent to 1 lb. on every square inch of the piston-area, and that the speed of the engine is at the rate of 1 revolution per second. In order to suit different conditions as to the crank-radius and the weight and speed of the reciprocating-parts, so long as the ratio of the connecting-rod to the crank remains the same, 3 to 1, the Diagram Fig. 2 has only to be drawn to a different vertical scale, all the ordinates being increased or diminished in the same proportion.[1] This proportion is determined by a simple rule, which may be thus stated: Multiply together the crank-radius in feet, the weight of the reciprocating-mass estimated in lbs. per square inch of piston-area, and the square of the number of revolutions per second. The product is a coefficient by which each of the ordinates of the inertia-diagram has to be multiplied in each particular case.

Fig. 2.



If, for example, the weight of the piston, piston-rod, slipper or cross-head and connecting-rod be such as to amount to 2 lbs. per square inch of piston-area; if the crank-radius be 9 inches or $\frac{3}{4}$ foot, and if the engine makes 4 revolutions per second, then $\frac{3}{4} \times 2 \times 4^2 = 24$ is the coefficient by which, in this particular case, each of the ordinates of the curve in Fig. 2 has to be multiplied in order to give the correct form of the inertia-diagram. Fig. 3 shows the curve thus computed applied to an indicator-diagram produced by steam at considerable initial pressure, cut-off at about one-third of the stroke, and expanding during the remainder of the stroke. The shaded portion of the figure represents the distribution of pressure throughout the stroke, after subtraction and addition of the pressures due to inertia; and thus it appears that the effective pressure available for external work is not very greatly varied throughout the stroke, notwithstanding the great alteration of the steam-pressure on the piston.

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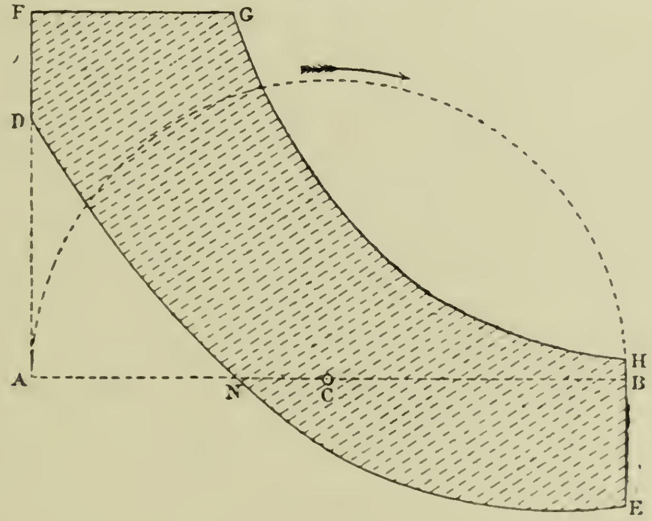
[1] The left-hand side of Fig. 2 has been foreshortened to admit of the Fig. coming within the width of the page.

This illustration will suffice to show how, by a proper adjustment of the reciprocating-mass, the crank-radius and the speed, to the inital pressure, the cut-off and the expansion of the steam, the inertia of the moving parts may be utilized in equalizing the action of a rapidly-moving engine. Thus it appears that reciprocation, so far from being objectionable, is of great and positive advantage, the alternate acceleration and retardation of the reciprocating-mass fitting admirably in with, and to a great extent compensating, the variations of pressure shown on the indicator-diagram.

Fig. 3.



The inertia-curve, illustrated by Figs. 2 and 3, is, as stated above, that which results from the use of a connecting-rod three times the length of the crank-radius. Where the connecting-rod is longer, the curve approaches more nearly to the straight line D C E in Fig. 1. With a shorter connecting-rod the curve departs farther from the straight line, and might be made to correspond more nearly with the indicator-curve than that which appears in

Figs. 2 and 3. If, for instance, the proportion of the connecting-rod to the crank were 2 to 1 instead of 3 to 1, the initial ordinate A D would be increased by about one-eighth in height, and the final ordinate B E would be reduced by about one-fourth in depth. Generally if c express the ratio of the connecting-rod to the crank, the initial ordinate A D is proportional to $(1 + \frac{1}{c})$, and the final ordinate B E is proportional to $(- 1 + \frac{1}{c})$. Thus, for different proportions of connecting-rods to cranks, the initial and final ordinates are as follow :—

c, Ratio of connecting-rod to crank	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$	4	∞
Initial ordinate A D . .	1·500	1·400	1·333	1·286	1·250	1·00
Final „ B E . .	−0·500	−0·600	−0·667	−0·714	−0·750	−1·00

A very short connecting-rod is objectionable on account of the great lateral strain thrown by it on the slipper or crosshead-guides; but even this disadvantage may be more than made up in high-speed motors by the great equalization of the effective pressure throughout the stroke, resulting from the parallelism of the inertia-curve to that drawn by the indicator, this parallelism being increased by the use of a short connecting-rod.

In what has preceded on the subject of reciprocation, it has been assumed that the reciprocating-mass moves horizontally, or that, if it moves vertically, it is balanced. Should it not be balanced, the weight, when it is acting favourably, adds to the area of the diagram by transferring the inertia-curve that forms its base a little lower down, and when it is acting unfavourably it reduces the area by transferring the base a little upwards. When the moving mass is light as compared with the acting pressures, the variation of the up- and the down-strokes, due to weight, may be disregarded. But when, in order to take full advantage of inertia as an equalizing agent, the weight of the reciprocating-mass is largely increased, the alternation of upward and downward movement causes an inequality, which may be serious in a motor applied to such a purpose as that of driving electric-machinery, where great uniformity of action is desirable and even essential.

In some reciprocating-engines, which are said to have succeeded well as electric-motors, there are two vertical single-acting cylinders having their pistons connected to opposite cranks on a shaft below them. In this case the reciprocating-weights are completely balanced, and the strain at the joints is always in one direction, that of thrust. Moreover, as only one of the pistons acts at one time, it has to deal with the inertia, not only of itself and its connections, but also with that of the other piston and its connections, so that practically the reciprocating-mass acting by inertia is always doubled. The principal objection to this form of engine is that there is nothing except the fly-wheel to get over the dead-points that occur twice in every revolution. In order to avoid the inequalities of movement resulting from the recurrence of these dead-points, probably the best form of engine would be an arrangement of three vertical single-acting cylinders having their pistons connected to a three-throw crank below. This arrangement, while it would provide well for continuous action, and would also satisfy the conditions of balancing the moving parts and of maintaining strain in one direction at the joints, would not utilize to the full extent the equalizing effect of inertia, as may readily be seen by constructing a diagram showing the simultaneous action of the three sets of reciprocating-parts.

If, in what has preceded, the equalizing effect of inertia has been correctly traced, a question may arise as to whether there is any advantage in working engines in the compound way. It is undoubtedly the fact that, when high-pressure steam is admitted in small quantity into a cylinder of large area, and then expanded throughout the greater part of the stroke, the variation of pressure, if there were nothing to compensate it, would be very objectionable. If the moving parts were light, there would at first be a severe shock, and at last a considerable drag. It has, therefore, generally been preferred to employ two cylinders of different dimensions, directing the high-pressure steam first on a piston of small area, and then, after a certain amount of expansion, directing the steam at reduced pressure on a piston of large area. This arrangement involves almost double work in respect of cylinders, slides, pistons and their connections, and necessarily adds to the amount of cooling-surface and to the risks of leakage. If, instead of providing cylinders in duplicate, a considerable addition were made to the weight of the reciprocating-parts for one cylinder, the inertia of the increased mass would to a great extent neutralize the inequality of the acting pressure. The best way of accomplishing this would be to increase the mass of the piston, being the part that is directly and immediately acted on by the steam-pressure. With a very heavy piston to be put in motion, accelerated, retarded and brought to rest in each stroke, no shock would be occasioned by the impetus of the high-pressure steam, and no drag from the great fall of pressure. The inertia of the piston itself might be made sufficient to produce complete equalization of effective pressure throughout the stroke, by first absorbing the excess of pressure for its acceleration, and afterwards restoring the equivalent by its retardation.

In the form of compound-engine largely used for marine propulsion, where the high-pressure and the low-pressure pistons act on cranks at right-angles to one another, there is considerable equalization of acting-pressure throughout each revolution, but the inertia has comparatively little equalizing influence. It may be open to discussion whether, by the use of two equal cylinders of mean area, each provided with a very heavy piston, so that full advantage might be taken of inertia as an equalizing agent, results as good as those obtained from the compound-engine might not follow.

Within the limits of a Paper like the present, it has not been possible to do more than take a cursory view of the subject of high-speed motors, which is a large one, involving many questions that ought to be dealt with in detail. The intention of the Author has been chiefly to direct attention to a few leading points, which seemed to him to demand more consideration than has generally been given to them. On these points certain conclusions present themselves, which may be thus summarised:—

1. For high-speed motors, engines of the rotary kind present no special advantages as compared with direct-acting reciprocating-engines.

2. Reciprocation, instead of being an evil, may, by proper adjustment of mass and speed, be made a very effective equalizing agent, particularly when there is great variation in the acting pressure.

3. Single action is to be preferred in high-speed motors in order to avoid the shocks resulting from alternations of strain at the working joints.

The Paper is accompanied by three diagrams, from which the Figs. in the text have been engraved.

[APPENDIX.

APPENDIX.

NOTE AS TO INERTIA OF THE RECIPROCATING-MASS IN A RECIPROCATING-ENGINE.

Referring to Fig. 1 :—

Let r = crank-radius C P;
a = uniform angular velocity of crank;
t = time from beginning of stroke at A until the crank-pin reaches P;
s = A R, the portion of stroke performed in time t;
i = Q R, the ordinate representing accelerating-pressure at time t.

Then $a\,t$ = A P, the arc traversed by the crank in time t.

$$s = r\,(1 - \cos a\,t) \quad \ldots \quad (1)$$

$$\frac{d\,s}{d\,t} = a\,r\,\sin a\,t \quad \ldots \quad (2)$$

$$\frac{d^2 s}{d\,t^2} = a^2 r \cos a\,t = \text{accelerating force} \quad \ldots \quad (3)$$

As the mass accelerated is constant, the pressure i producing acceleration is proportional to the accelerating-force;

hence $\quad i : r\cos a\,t \quad \ldots \quad (4)$

or $\quad$ Q R : R C $\quad \ldots \quad (5)$

Hence all the ordinates such as Q R are bounded by an inclined straight line D C E.

All the ordinates on the one side of the centre, which are proportional to cosines of arcs from 0° to 90° are positive, indicating acceleration. The ordinates on the other side of the centre, which are proportional to cosines of arcs from 90° to 180° are negative, indicating retardation.

Referring to Fig. 2 and retaining the above notation—

Let $\quad c$ = the ratio of the connecting-rod to the crank-radius,
so that $\quad c\,r$ = length of the connecting-rod,
then $\quad$ T V = T A + A C − C X − V X,

or
$$s = c\,r + r - r\cos a\,t - \sqrt{c^2 r^2 - r^2 \sin^2 a\,t}$$
$$= r\left\{c + 1 - \cos a\,t - \sqrt{c^2 - \sin^2 a\,t}\right\} \quad \ldots \quad (6)$$

$$\frac{d\,s}{d\,t} = a\,r\left\{\sin a\,t + \frac{\sin a\,t\cos a\,t}{\sqrt{c^2 - \sin^2 a\,t}}\right\} \quad \ldots \quad (7)$$

$$\frac{d^2 s}{d\,t^2} = a^2 r\left\{\cos a\,t + \frac{\cos^2 a\,t - \sin^2 a\,t}{\sqrt{c^2 - \sin^2 a\,t}} + \frac{\sin^2 a\,t\cos^2 a\,t}{(c^2 - \sin^2 a\,t)^{\frac{3}{2}}}\right.$$
$$= a^2 r\left\{\cos a\,t + \frac{c^2(\cos^2 a\,t - \sin^2 a\,t) + \sin^4 a\,t}{(c^2 - \sin^2 a\,t)^{\frac{3}{2}}}\right\} \quad \ldots \quad (8)$$

In the case where c is infinite, the fraction in (8) vanishes, and the equation becomes simply $\frac{d^2 s}{d\,t^2} = a^2 r\cos a\,t$, as already appears in (3).

When $a\,t = 0°$, in which case $\sin a\,t = 0$ and $\cos a\,t = 1$, the quantity within brackets is $1 + \frac{1}{c}$, which, multiplied by a proper coefficient (as hereafter computed), gives the length of the initial ordinate of the inertia-curve.

When $a\,t = 180°$, in which case $\sin a\,t = 0$, and $\cos a\,t = -1$, the quantity within brackets is $-1 + \frac{1}{c}$, which gives the length of the final ordinate of the curve.

For determining the coefficient, to give pressure—

Let $n =$ the number of revolutions per second,

$\therefore a = 2\,\pi\,n$.

Let $w =$ weight (lbs.) of the reciprocating parts per square inch of piston-area;

$g =$ force of gravity (32 feet per second); then r being measured in feet, the accelerating-pressure per square inch of piston-area is as

$$\frac{w}{g}\,(2\,\pi\,n)^2\,r, \text{ or as } \frac{(2\,\pi)^2}{g} = 1{\cdot}23 \text{ nearly, the coefficient is } 1{\cdot}23\,w\,r\,n^2\,. \quad (9)$$

In the case where $w = 1$ lb., $r = 1$ foot, and $n = 1$ per second, the coefficient is simply 1·23, and from (8)—

$$i = 1{\cdot}23 \left\{\cos a\,t + \frac{c^2\,(\cos^2 a\,t - \sin^2 a\,t) + \sin^4 a\,t)}{(c^2 - \sin^2 a\,t)^{\frac{3}{2}}}\right\} \quad . \quad . \quad . \; (10)$$

By giving $a\,t$ a number of successive values 0°, 30°, 60° up to 180°, the corresponding values of i can be computed, and the inertia-curve can be drawn.

These values are—

$a\,t =$	0°	30°	60°	90°	120°	150°	180°
$i =$	1·640	1·282	0·411	−0·435	−0·819	−0·848	−0·820

As these ordinates may be drawn to any scale, still retaining their relative values, it is only necessary, for any weight w, radius r and speed n, to select a scale such as is equivalent to the multiplication of each ordinate by the coefficient $w\,r\,n^2$, the product of the weight in lbs., the crank-radius in feet, and the square of the number of revolutions per second.

(*Paper No. 2113.*)

"Modern Continuous-Current Dynamo-Electric-Machines and their Engines."[1]

By GISBERT KAPP, Assoc. M. Inst. C.E.

I. GENERAL TENDENCY OF IMPROVEMENTS.

THE early history of dynamo-machines differs from that of most other great mechanical inventions in this respect, that here the natural order of development has, to some extent, been reversed. As a rule, new mechanical appliances have been invented by men thoroughly conversant with the wants of each case, who were either striving to improve well-known, but imperfect, methods already existing; or else to devise some entirely new apparatus intended to be used as tools to perform work which until then had been done by hand. This circumstance, that the invention was made for some specific purpose, enabled the designer to cast his ideas at once into a practical form; which, although it might have been defective from a scientific point of view, had yet the great merit of doing, in some sort of way, the work for which it was intended. Further improvement followed, as a natural consequence of the experience obtained with that particular class of apparatus, and of the application of scientific principles. This order of development has been reversed in the case of the dynamo-machine. Here science was first, and practice only followed after an interval of many years. Although the motor invented by Professor Pacinotti was perfectly successful, and not only contained all the important features of a continuous-current magneto-machine, but was even described by its inventor as applicable for the production of continuous currents, yet, as there was at that time no demand for powerful electric currents, it remained undeveloped and almost forgotten until seven years later Mr. Gramme re-discovered the principle of the Pacinotti armature. The development of the modern dynamo-machine dates from that period, viz., the year 1871. The general tendency of this development has been, in the first place to transform a scientific toy into a substantial piece of mechanism by experience, practical skill and good workmanship; and in the second place to improve the proportions of various parts by the application of scientific knowledge so as to economize material and increase efficiency. The original Gramme machines, and their imitations, were mechanically very imperfect. It would almost seem as if their constructors had failed to realize the magnitude of the mechanical forces entering into their action. Whether a system of measurements based upon units so small, that millions and even hundreds of millions were required to represent quantities ordinarily dealt with in practice, or the absence of any system of measurement contributed somewhat to the neglect of designing these machines on rational principles, may be a moot point. In all probability the designers did not trouble to make any calculations as to strength of parts until, through repeated failures, they became aware that, instead of having to deal with a few dynes and ergs they had in reality to deal with many pounds and horsepowers. The construction of the original Gramme armature is so well known that it would be waste of time to enter into a detailed description of it. Suffice it to recall to mind that the core, after being wound with insulated copper wire, was driven over a conical wooden centre by which it was then fixed to the shaft. All the work is done by the external portion of the wire, which was fastened to the surface of the core by friction only. The driving-power in the spindle had therefore to be transmitted to the working wires by a series of frictional attachments, viz., between the wooden centre and the inner coils, between the inner coils and the core, and finally between the core and the outer coils. It will be evident that, in this manner, only a very small turning moment, or torque, could be transmitted, since any considerable local pressure between the surfaces in contact would have destroyed the insulation of the copper wire. The original machines were, however, rather bulky in comparison to the power they could absorb, and the armature contained a sufficient amount of surface, along which an attachment by simple friction could be made without putting an excess of local pressure on the wire; and this explains why failures did not occur more frequently. As, through improvements in field-magnets and general proportions, the power of dynamos, as compared to their bulk, was more and more increased, the old attachment by friction became unreliable, and in all good modern machines more positive methods have been substituted. The importance of securely fastening the external, or working, wires on the armature cannot be over-estimated. It is not only necessary to provide for the strains in ordinary regular work, but also for strains caused by a short-circuit between the main leads, such as might be the result of an accident, or as might occasionally occur in hand-fed arc lights. The resistance of modern dynamos is so low, that the excess of current resulting from a short-circuit is very considerable (from five to fifteen times the normal current), and since the mechanical strain on the working wires is approximately proportional to the current, it will be evident how necessary it is to hold these wires firmly in place.

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[1] The discussion upon this Paper was taken together with that upon the preceding one.

Other improvements might be classified under the following headings: 1st. Field-Magnets; as to the material of the core and the pole-pieces, their general proportions and arrangement, and the number of poles. 2nd, Armature; as to methods of supporting the core, its shape and proportions, its subdivision and internal ventilation; also the disposition of the copper conductor, with a view to obtaining the strongest possible current without undue heating; and the means adopted to reduce the speed; 3rd. Other points; as improved insulation, construction of the commutator and brushes.

With regard to field-magnets the substitution of soft, well-annealed, wrought-iron for cast-iron results in an increase of electromotive force of from 20 to 30 per cent. Magnet-forgings of good hammered scrap-iron (not Swedish) can now be obtained at from £16 to £17 per ton, whereas cast-iron magnets would cost about £11 per ton. The difference between the two of £5 to £6 per ton, is well worth the gain in electrical output, and probably in future all low-tension dynamos will have wrought-iron magnets. In very high-tension machines used for arc-lighting, cast-iron magnets have hitherto been almost exclusively used; but in this case it is not so much a question of obtaining the greatest possible electrical output with a given weight of material, as to obtain a current suitable for a large number of arc-lamps coupled in series. The low saturation-point of cast-iron, which is the very quality making it almost unfit for self-regulating low-tension machines, renders it particularly suitable for arc-machines, where constancy of current rather than of electromotive force is required. As regards the proportions of magnets, and the number of poles, the general tendency in cylinder-machines is to employ only one pair of poles, but to increase the power of the machine by making these poles large and very strong, whilst in disk-machines the tendency is to employ two or more pairs of smaller poles.

The armature-core, in all modern machines of the Gramme type, is supported by metal arms in a positive manner, rendering slipping of the core impossible. Air is admitted between the arms to the inner surface of the ring, and in some cases even to the inner surface of the core, and through ventilating spaces in the core to its outer periphery, thus effectually cooling the core and winding. In consequence of this arrangement, it has been found possible to considerably increase the density of current in the armature-wire without increasing its temperature beyond a safe limit. In the early days of dynamo-construction, it was customary to fix the density at from 1,200 to 2,000 amperes per square inch, according to the size of the wire, but irrespective of the capacity of the armature to dissipate heat. This was evidently a most unscientific way of designing armatures. If this rule were followed in an armature having but two layers of wire, very probably the temperature after many hours' working would remain practically constant and within a safe limit. But if the same rule were used when designing an armature having ten layers of wire, the temperature would rise much higher, since five times as much heat would be developed in unit time, whilst the cooling-surface would not have been materially increased. The only rational way of fixing the density of the current in the armature wires is by allowing the heat developed, when the full current is passing, to be proportional to the total surface of wire exposed to the cooling influence of the surrounding air. The determination of the exact proportion is purely a matter of experience. Considering the great difference which exists in the surface-speed of armatures and in structural arrangements, which might more or less prevent a frequent renewal 'of the surrounding air, it is difficult to give figures applicable to all cases. From a number of machines by different makers which have come under the Author's notice, and from long trial-runs with his own dynamos, he has found that with a surface-speed of 2,000 to 3,000 feet a minute, and an armature to which air is freely admitted, the cooling-surface provided should not be less than 0·8 square inch, and need not be more than 1 square inch, for every watt transformed into heat in the armature-wires. A similar rule holds good for the external surface of magnet-coils; only, since in this case the air surrounding the coils is comparatively quiescent, a somewhat larger surface is required, the figures being 1·5 to 1·8 square inch per watt expended in exciting the field-magnets.[1] The average density of current allowed now in the armature wires of modern machines is about 2,500 amperes per square inch of cross-section of wire, but much greater densities are sometimes met with. In some dynamos which recently came under the Author's notice, the density was 6,000 amperes. The wire on the armature formed a single layer only, but no provision had been made for admitting air to the core. The cooling-surface provided in these dynamos is only 0·22 to 0·30 square inch per watt transformed into heat within the armature-wires, but the Author has not had an opportunity of seeing these machines tested under a full load and during a long run, and is therefore unable to say whether the cooling-surface of 0·30 square inch per watt has been sufficient in this case.

It will be evident, the greater the density of current that can be allowed without danger of injurious heating, the greater is the electrical output which a given weight of copper on the armature will yield. The immediate practical result of better ventilation is, therefore, that with modern dynamos a greater number of amperes are produced for every lb. of copper used in the construction of the armature. On the other hand, it has been found possible, by making the magnetic-field stronger, to increase the electromotive force induced per unit-length of armature-conductor, and thus current and electromotive force have been simultaneously increased, and their product, or the electrical output measured in watts obtained per lb. of copper, is now two or three times greater than in machines built a few years ago. As a natural consequence, the price of dynamo-machines has been considerably reduced.

II. Theoretical Considerations.

Although a very large number of apparently different types of dynamos have, within the last few years, been tried or patented, those machines which have been successful in actual work are not numerous, and in principle they are much alike. All continuous-current dynamos of practical importance at the present moment can be grouped under three headings:—

1. Dynamos with drum-armatures wound on the Hefner-Alteneck principle.

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[1] Professor Forbes' formula would in this case give a rise of temperature of 82° and 68° Centigrade respectively. It is quite certain that the rise is not so great. The explanation of this might be that other parts, besides the surface of the coils, dissipate heat.

2. Dynamos with cylindrical-armatures wound on the Gramme principle.

3. Dynamos with disk-armatures wound on the Schuckert principle.

The first type is represented by the machines of Siemens, Edison, Weston, and, with a slight modification, by those of Immisch and Houston-Thomson. The second type is represented by a large number of makers, including Elwell and Parker, Crompton, Goolden and Trotter, Mather and Platt, Allen, Jones, Paterson and Cooper, and others. The third type is not so strongly represented in this country, the makers being the Brush Company, the Gülcher Company, and a few manufacturers of less importance. From a scientific point of view all these machines, notwithstanding their apparently great disparity in design and general arrangement, can be treated in a similar manner.

In all cases the electromotive force created within the coils of the armature can be expressed in volts by the use of the formula[1]—

$$E\,a = z \cdot N\,t \cdot n \cdot 10^{-6},$$

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[1] This formula is deduced from the well-known expression for electromotive force given in absolute measure. If a wire b centimetres long be moved with a velocity of v centimetres per second, parallel to itself and at right-angles to the lines of a uniform magnetic-field of intensity F, so as to cut $F\,b\,v$ lines per second, the difference of potential created between the ends of the wire is $F\,b\,v\,10^{-8}$ volts. If the wire instead of being straight were bent into any shape, and if the intensity of field were not uniform, the result would still be the same, provided that $F\,b\,v$ represents now, as before, the total number of lines cut in one second. Imagine now $\frac{N\,t}{2}$ such wires laid side by side over the core of an armature, so as to cover one-half its external surface, and let their ends be so connected that the electromotive force developed in each wire is added to that of its neighbours, the connections, however, being removed from the influence of the lines of force, which can be done by placing them inside the core; then the difference of potential between the free ends of the first and the last wire will be the sum of the electromotive forces developed in each wire. Let D be the diameter of the armature, and let the speed of rotation be so small that the speed of the wires is exactly equal to their distance, then $v = \frac{\pi D}{N\,t}$; and, if it be assumed that the intensity of the field, instead of being uniform all around the armature, varies in any irregular manner, being F_1, F_2, F_3 and so on, in the positions occupied at any given moment by the first, second, third wire and so on, there result for the electromotive forces created in these wires the equations—

$$E_1 = F_1\,b\,\frac{\pi D}{N\,t}\,10^{-8}$$

$$E_2 = F_2\,b\,\frac{\pi D}{N\,t}\,10^{-8}$$

$$E_3 = F_3\,b\,\frac{\pi D}{N\,t}\,10^{-8}$$

where z represents the number of lines of force passing between the armature and one of the magnet-poles, $N\,t$ the number of turns of conductor counted all round the external circumference of the armature, and n the speed in revolutions per minute. A few words of explanation regarding z are necessary. The formula is only correct if these lines pass entirely along the body of the core, so as not to be cut by any other wires but those on the external periphery, or on the end-faces of the armature. In drum-armatures this condition is evidently always fulfilled, for there are only external wires on the core. In cylinder- or disk-armatures this condition will only be fulfilled if the radial depth of the core is sufficiently great to accommodate all the lines which emanate from one field-magnet pole. If this be not the case, then some of the lines will be forced across the central space in the armature, and will be lost for the production of electromotive force. As regards the system of units in which z is expressed, it will be seen from the foot-note that each of these lines is equivalent to 6,000 lines in absolute measure. This basis has been adopted merely for convenience, in order to be able to use manageable figures when expressing the number of lines which can pass through 1 square inch of armature-core, but it in no way affects the final result.

When a current is permitted to flow through the armature, the internal electromotive force is, strictly speaking, somewhat lower than that given by the formula, on account of the magnetizing-power of the armature-current, and of self-induction in the armature-coils. In modern machines the field is, however, so strong, and the number of turns coiled on the armature is so small, that for practical purposes this reduction of electromotive force can, in most cases, be neglected. The desideratum in designing a dynamo is of course to obtain the highest possible electromotive force. This

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Now E_1 is evidently equal to the total number of lines entering the armature between the first and second wire; E_2 equals that number between the second and third wire, and so on. The sum of all these numbers is therefore equal to the total number of lines emanating from one magnet-pole and entering the half circumference of the armature. Let z be that number, then the total electromotive force developed at the speed of $v = \frac{\pi\,D}{N\,t}$, $E\,a = z\,10^{-8}$. If the speed were increased to n revolutions a minute, v would be $\pi\,D\,\frac{n}{60}$, and the electromotive force would be increased in the proportion of $1 : \frac{N\,t\,n}{60}$. Therefore $E\,a = z\,N\,t\,\frac{n}{60}\,10^{-8}$, which can also be written in the form $E\,a = \frac{z}{6{,}000}\,N\,t\,n\,10^{-6}$. By putting $z = \frac{z}{6{,}000}$ the expression used in the text is obtained.

can be done in three ways. Either by employing a strong field (z large), or a large number of turns on the armature, or a high speed of revolution. As regards the latter expedient, there is evidently a limit beyond which it would not be safe to go. In belt-driven machines this limit is imposed by the strength of materials in those parts of the machine which are subject to the action of centrifugal force, and by the general inconvenience which the use of very high speeds entails. For these reasons dynamos are generally so speeded, that the circumferential velocity of the armature shall not exceed 3,000 feet a minute in the case of drum- or cylinder-machines, and 5,000 feet a minute in the case of disk-machines. When driven direct their speed is naturally limited by that of the engine. The other expedient, of increasing the number of turns on the armature, is also limited by serious practical difficulties. If the armature be increased generally in order to get on sufficient wire, the machine becomes too costly; if the wire be wound on in too many layers, the trouble of self-induction, sparking at the brushes and heating of the coils, will generally more than counterbalance the gain in electromotive force, especially since the increased resistance of the armature results in a reduction of efficiency; and also because on account of the greater distance between the core of the armature and the polar-surfaces, more exciting power, and therefore a greater expenditure of energy, is required to maintain the field. Within reasonable limits, however, the multiplication of convolutions on the armature is perhaps the most economical way of increasing the electromotive force, at any rate as far as cost of manufacture is concerned. If, on the other hand, it is desired to obtain the highest possible electrical efficiency, the employment of a very strong field becomes necessary. By using the expression "strong field" the Author does not mean to convey the idea of a very intense magnetization pushed towards the saturation-limit, but simply that of a large number of lines, and a sufficient mass of iron in the magnets and armature to accommodate these lines. This is the principle on which nearly all good modern dynamos are designed.

Since iron, be it of the softest quality, can only accommodate a limited number of lines of force per square inch, it follows, that the field is proportional to the cross-sectional area of the armature-core. It is thereby supposed that the exciting power on the field-magnets is in all cases sufficient to fill the armature-core to the same density with lines. This proposition granted, the question arises, what is the best type of armature in order to produce a maximum of electromotive force with a minimum length of conductor? The great practical importance of this matter becomes evident if it be considered that the fewer the turns, the larger can be the wire employed, and the greater can be the current obtained from a given size of armature. In comparing the three principal types above specified in this respect, it is necessary to make at the outset certain assumptions regarding proportions and circumferential speeds, in accordance with the generality of facts found in actual practice, and thus to establish a fair basis of comparison. As regards the speed, it will be evident that disk-armatures have an advantage over those of the drum- or cylinder-type, inasmuch as most of the strain produced by centrifugal force takes place in the direction of the length of the wire instead of transversely to it, rendering it in many cases unnecessary to employ binding hoops. At the same time the bearings are comparatively near, and vibrations of the spindle are not so likely to take place, as in drum- or cylinder-armatures, where the bearings are necessarily further apart; and if vibrations occur, they are less dangerous because their direction is parallel to the polar-surfaces. It is found, therefore, that the speed of disk-armatures is about 60 per cent. higher than that of the other two types. The circumferential speed of the drum and cylinder will be assumed to be 3,000 feet a minute, and that of the disk 5,000 feet a minute, figures which agree very well with general practice. It will further be assumed that the radial depth of core in the cylinder and disk is one-fifth of the diameter. This figure also agrees well with common usage, and is to a certain extent fixed by the necessity of providing between the arms of the core sufficient space for the accommodation of the inner coils of the winding. In drum-armatures the radial depth of core may be taken as one-third of its diameter, the central portion of the core being occupied by the spindle, which, being of steel and solid, cannot be considered to contribute anything to the magnetic conductivity of the core. As regards the length of core measured parallel to the spindle, an infinite variety is, of course, possible. But in order not to multiply examples the Author has assumed only a few cases, which are those more nearly approached in actual dynamos, or which may be considered as possible limits. These are the following:—

1. Drum, very long in comparison to the diameter.
2. Drum, length equal to the diameter, rectangular core 3 : 1.
3. Cylinder, very long in comparison to the diameter.
4. Cylinder, length equal to the diameter, rectangular core 5 : 1.
5. Disk, length equal to the radial depth, square core.
6. Disk, length equal to the radial depth, circular core.
7. Disk, length equal to one-fifth the radial depth, rectangular core 1 : 5.

The next assumption to be made is that of the density of lines per square inch of core-area. In modern dynamos working at their full output this figure lies between 15 and 25. In some exceptional cases higher figures may be obtained; and in one instance the Author has found 27·4 lines for every square inch of iron in the core; but, in this case, the clearance between the armature and the polar-surface was only $\frac{1}{16}$ inch, and it was found necessary for the safety of the machine to bore out the magnets by $\frac{3}{32}$ inch, after which the density fell to 25 lines. It is well known that the mass of iron in the core must be considerably subdivided in order to avoid internal heating and waste of power; and in estimating the density of lines, only the space actually occupied by iron must be taken into account. When the armature is built up of disks or iron tape, the insulation between the subdivision occupies about 10 per cent. of the total space; when wire is used the space wasted is $1 - \frac{\pi}{4} = 21$ per cent. Not to complicate the problem, it will, however, be assumed to be possible to make the insulation so thin as to be negligible. It will also be assumed that the length of one turn of copper-conductor wound over the core is equal to the perimeter of the core, thus neglecting external insulation and thickness of conductor, and the length of wire, required for the connections between the armature and the commutator will be neglected. In the Hefner-Alteneck armature the length of wire crossing the ends of the drum is taken as 1·5 time its diameter. Assuming that in all cases the exciting power of the magnets be so regulated as to produce a density of 20 lines per square inch of armature-core, the length of wire required to produce 1 volt of electromotive force can now be calculated. The calculation is merely an algebraical operation presenting no difficulty, and need, therefore, not be given at length. The result is as follows:—

1. Drum, long,	requires	6·5	inches per volt.
2. Drum, short	,, ...	16·4	,,
3. Cylinder, long	,, ...	21·8	,,
4. Cylinder, short	,, ...	26·1	,,
5. Disk, square core	,, ...	26·3	,,
6. Disk, circular core	,, ...	26·3	,,
7. Disk, flat core	,, ...	78·8	,,

These are minimum values, and refer only to the electromotive force within the armature. For the value available in the external circuit the proportion of length of conductor per volt would be somewhat greater, as a certain portion of the internal electromotive force is lost on account of the resistance of the armature, and also of the exciting coils, if the machine be series- or compound-wound. The above Table shows the great superiority of the drum over any other armature. The long cylinder comes next, but is not generally employed in modern machines on account of vibration in the spindle. The tendency is rather to employ short cores in order to reduce the distance between the bearings. Cores of equal length and diameter are very commonly used. These are, as will be seen from the list, equivalent to disk-armatures with square or circular cores. If the core of the disk be a rectangle, the sides of which are as 6 is to 10 (a proportion generally used in practice) 34 inches of wire are necessary to produce 1 volt. In this case the disk is therefore not so good as the short cylinder. The flat ring, which was extensively used in the early days of disk-machines, is still worse, requiring three times as much wire as the short cylinder. Opinions have completely changed with regard to certain questions connected with the construction of dynamos. At one time a very narrow and deep ring was considered the best possible form to be given to the core of a Gramme armature, as thereby a great expansion of polar-surface could be obtained, thus bringing nearly the whole of the wire under the direct influence of the magnets. This idea was embodied in the early designs of Schuckert and Gülcher. But experience and theory have since shown that it is a fallacy, and the latest dynamos, built by the Gülcher Company have a circular core; whilst the Brush Company, instead of employing a flat ring, use a rectangular core of considerable width. It has also been found that an undue expansion of pole-pieces is useless, and may do harm. The poles have, therefore, been made narrower, and in consequence, two and more pairs can be arranged round the disk, thus increasing the number of points on the commutator, at which the current can be taken off. By this means the armature is subdivided into several parallel circuits, each giving the original current. It should be noted that the increase of total current obtainable by this arrangement need not necessarily be accompanied by a decrease of electromotive force. If it be possible so to arrange and excite the field-magnets that the original density of lines in the core of the armature is maintained, then the electromotive force will also remain the same. At first this result might seem somewhat startling, since with the multiplication of poles the number of armature-coils under the influence of each pole is proportionately reduced. But, on the other hand, the time required by each wire to pass from one pole to the other is also reduced in the same proportion, and thus the final result is not affected. From these considerations it would appear as if disk-armatures had an enormous advantage of increased output over cylinder-armatures, and on purely theoretical grounds this is so. But there is one practical difficulty, which to a great extent counteracts their good theoretical qualities. From the very nature of their construction the lines of force must enter the core in all possible directions. Some enter parallel to the axis, others at a more or less acute angle, and others again at right-angles. The latter are those which enter on the external circumference, and the former are those which enter at points nearer the axis. To prevent heating, the surfaces between the subdivided portions of the core should always be parallel to the direction in which the lines enter, but when the lines enter at all possible angles the fulfilment of this condition presents some practical difficulties. This is probably the reason why, in cores built up of coils of iron tape, the cross-section is a rectangle and not a square, although the latter would be preferable on theoretical grounds. But by making the width of the tape too great, the outer layers, which are pierced by the lines on the broad surface, would become dangerously hot. It is therefore necessary to employ a rather narrow core, and in some cases to further subdivide even that by turning grooves into it on the outer periphery. If the armature be constructed of flat-iron washers placed side-by-side, instead of the coiled tape, it is rather worse, as the heating then takes place on both side-faces instead of at the periphery. These difficulties may to a certain extent explain the fact, that so few makers have adopted the disk as their standard type of armature, whereas cylinder-armatures are much more in favour. Patent rights, which evidently debar many makers from adopting the drum-type, although best of all, cannot have an influence in the matter; for both the cylinder and the disk are simply different forms of the ordinary Gramme armature, the use of which is open to all.

It has been mentioned above that the electromotive force developed in any of these armatures is proportional to z, the total number of lines emanating from one pole and entering the armature-core. The number of lines will evidently depend on the size and shape of the machine taken as a whole, on the quality of iron used in the field-magnets and armature, and on the magnetizing power. By plotting magnetizing-powers on the horizontal and the corresponding number of lines on the vertical, a curve is obtained similar in shape to the ordinary characteristic of a series-wound dynamo. Instead of current and electromotive force, there is now magnetizing-power and strength of field. If the ordinary characteristic be employed, it is always under the supposition that throughout the range of the curve the speed of the dynamo is kept constant; if a different speed be adopted there must be a new curve for it. In the "characteristic curve of magnetization" speed does not enter, so that the same curve can be used for all possible speeds; it can also be used irrespectively of the manner in which the magnetization is effected, whether by series-coils, shunt-coils, or from an independent source, and for these reasons it will be more generally useful than the ordinary characteristic.

The relation between the strength of field z and the exciting power P is of great practical importance; and if it were possible to determine it for any given size and type of dynamo without the costly expedient of previous experiments, the problem of designing dynamos on a scientific basis would be solved. In the formulas generally found in treatises on the subject, a number of coefficients are used which are not constants, but vary with each type of machine, and have to be determined experimentally for each case. In order to design a dynamo of a given type, it is therefore necessary to have recourse to experimental data obtained with a machine of precisely the same type; but if such be not available, success can only be achieved after a series of more or less costly failures. It will be admitted that this is a most unsatisfactory way of designing, and that it is highly desirable to obtain coefficients and formulas which should be applicable to all types, either existing or yet to be devised. Only on this condition will it be possible to foretell from the drawing of a dynamo what its performance is likely to be, and much costly experiment and rule of thumb will be avoided. As far as the Author is aware nothing has yet been published on the construction of dynamos, which would be of practical value to the manufacturer. Of theories there are more than enough, but the connecting links between pure science and practical work are still missing. At present, every maker has to work out his own theory and rules of construction, which he naturally keeps as secret as he can. As a step towards generalization of the subject, and in the hope that other information of a similar character may be elicited, the Author would here submit the formulas he is using in the designing of dynamos. In establishing them his aim has been to do away as far as possible with coefficients depending on the type of machines, and to put the expressions in a form sufficiently simple for practical use. He cannot say whether they will fulfil the conditions stated above of being universally applicable; and he considers them rather as a first attempt at getting rid of those vexatious coefficients which stand in the way of general formulas. The strength of field, or number of lines z, is the ratio of exciting-power P to magnetic-resistance R. In a dynamo-machine the latter consists of three parts—R_a that of the air-space, which is considered to be independent of the strength of field, and of the resistances of the armature-core (R_A), and the field-magnets (R_F), both of which are considered to increase with the strength of the field.

$$z = \frac{P}{R_a + R_A + R_F}.$$

For low degrees of magnetization this increase is so slight as to be negligible, and in that case z and P are simply proportional. This corresponds to the straight part of the characteristic curve near its origin. From comparing the curves of a large number of dynamos, the Author has found that up to a density of about 10 lines per square inch, either in the armature or the magnet-core, the total resistance of the magnetic circuit can be considered as constant. For higher degrees of magnetization, a "saturation term" must be added to R_A and R_F, which has the effect of increasing the total resistance from about 40 per cent. in self-regulating incandescent-light dynamos to 100 per cent. in arc-light dynamos. The large increase of resistance in the latter is due to the necessity of working with a strongly-saturated field, in order to obtain a slightly drooping characteristic, and thus enable the lamps to "feed" properly. It would be beyond the scope of this Paper, and would uselessly burden it with matters of secondary interest, if a mathematical dissertation of the saturation-term were included. The point of greatest practical importance is the magnetic resistance for low degrees of magnetization, for on that depends whether the machine can be self-exciting, that is, can work as a dynamo. The Author proposes, therefore, to call this the "critical magnetic resistance," and the corresponding exciting power the "critical exciting power." These values can be determined for any given dynamo, by working it at so low a speed that it will just self-excite, but lose its magnetism again on a slight reduction of speed. The Author has made a large number of such experiments with dynamos of different types and sizes, and in this way has collected data which enable him to find expressions for the quantities which make up the critical magnetic resistance. If the armature be of the cylinder or drum type,

Let δ represent the distance between the armature-core and the polar-surface;

$\lambda\, b$ represent the area of pole out of which z lines enter the armature, λ being the arc embraced, and b the length of armature;

$a\, b$ represent the area of armature-core actually occupied by iron, a being the radial depth;

A B represent the area of the magnet-core;

l and L represent the length of the magnetic circuits in the armature and the magnets respectively;

then, for a two-pole machine with only one horse-shoe magnet, such for instance as the Edison-Hopkinson dynamo,

$$R_a = \frac{1{,}440 \times 2\,\delta}{\lambda\, b},$$

$$R_A = \frac{l}{a\, b},$$

$$R_F = \frac{2\,L}{A\,B}.$$

In a machine with double magnets, such as the Siemens, Weston, Crompton, Mather and Platt, and others, each horse-shoe only supplies one-half the number of lines which pass through half the polar-surface, and along one side of the core only. For this case the values are—

$$R_a = 2\,\frac{1{,}440 \times 2\,\delta}{\lambda\, b},$$

$$R_A = \frac{2\,l}{a\, b},$$

$$R_F = \frac{2\,L}{A\,B}.$$

Alterations in these expressions, when applied to multipolar disk-machines, will readily suggest themselves, but need not be given at length.

These formulas are purely empirical,[1] and apply to dynamos with well-annealed wrought-iron magnets and armatures, composed of charcoal-iron wire, or disks. For machines with cast-iron magnets the coefficient for R_F is 3 instead of 2, and in the formula for z, $0 \cdot 8$ P has to be used instead of P.

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[1] The Author has used these formulas for some considerable time without at first troubling to inquire whether they have any scientific basis. Later, he has, chiefly by the kind assistance of Sir W. Thomson and Professor Lodge, been able

In dynamos, as generally constructed, the resistance of the air-space is always considerably greater than that of the magnets or armature. If two dynamos having equal armatures be compared, but one with a double and the other with a single magnet, the total magnetic resistance of the latter will be found only slightly more than one-half that of the former. The number of lines flowing through the single magnet is twice that of each horse-shoe of the double-magnet, and the exciting power in both machines is therefore approximately equal. But in the double-magnet machine this exciting power must be applied on each of the two horse-shoes, whereas in the single magnet machine it has to be applied on one horse-shoe of double the sectional-area. The length of wire required will therefore be as $2 : \sqrt{2}$; or, by the employment of a single magnet about 25 per cent. of the wire can be saved as compared with a double-magnet machine. This consideration has

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to find a scientific reason for them. This is the well-known formula for the field produced by a solenoid. Imagine a curved electro-magnet, the core of which shall have neither power to increase the number of lines created by the coil, nor to oppose any resistance to their flow; let the end faces be parallel, and separated by a distance $2\,\delta$, then the intensity of the field between the polar-surfaces is given in absolute measure by $\frac{4\,\pi\,P\,10^{-1}}{2\,\delta}$. Let S be the surface of each pole in square centimetres, then the total number of lines is $z = S\,\frac{4\,\pi\,P\,10^{-1}}{2\,\delta}$, which can also be written in the form $z = 4\,\pi\,10^{-1}\,\frac{P}{\frac{2\,\delta}{S}}$. Now $\frac{2\,\delta}{S}$ is a length divided by a surface, an expression similar in character to that used to calculate the electrical resistance of a conductor when its dimensions and the specific resistance of the material are known. In this case the specific magnetic resistance would be $\frac{10}{4\,\pi}$, and $\frac{10}{4\,\pi} \cdot \frac{2\,\delta}{S}$ would represent the magnetic resistance of the air-space. In a dynamo there are two air-spaces, each of thickness δ, and the polar-surface is $\lambda\,b$; therefore $\frac{2\,\delta}{\lambda\,b}$ multiplied by a constant coefficient gives the resistance of that portion of the magnetic circuit which lies in air. It is but reasonable to suppose that for those portions of the circuit which lie in the iron of the armature and the magnets a similar law holds good; hence the expressions used in the text. The relation between P, R, and Z bears a strong analogy to the well-known Ohm's law, as will be apparent if exciting power be substituted for electromotive force, number of lines for current, and magnetic for electrical resistance.

led the Author to adopt the form of magnets shown in Figs. 12 and 13. A doubt might possibly be entertained whether the one-sided application of exciting power would have the effect of disturbing the symmetry of the field, and cause sparking at the brushes. Experiment has shown that this is not the case. It has also shown that the electromotive force of the armature is practically independent of the position of the exciting power. The experiments were made by successively cutting out of circuit the top coil (Fig. 12), and the two side coils, and comparing the results with those obtained in ordinary working, when all three coils are in circuit. It was thus found that the electromotive force depends on the total exciting power, but not on the position of the exciting coils. That this is also the case in other machines is proved by the excellent results obtained by Messrs. Mather and Platt with their "Manchester" dynamo (Fig. 2). In this machine the exciting coils are placed over those portions of the magnets which in some other machines constitute the yokes.

A question of great practical importance is that as to the relations existing between the linear dimensions of a dynamo, its electrical output, efficiency, weight, and price. Assume that of two dynamos one has q times the linear dimensions of the other. They shall both be wound with the same number of turns of wire on the armature as well as on the magnets, but the diameter of the wire shall be as $1 : q$ on the magnets, and $1 : q^{\frac{3}{2}}$ on the armature. If it were possible to make the thickness of insulation and the clearance in the proportion of $1 : q$, then the diameter of the armature-wire would also be in the same proportion. But the thickness of insulation, and the clearance necessary for safe working, do not increase in the same ratio as the linear dimensions of the machine, so that the larger machine has the advantage of having more space to be filled by copper, and less to be wasted in insulation and clearance. The proportion of $1 : q^{\frac{3}{2}}$ will be found to agree fairly well with general practice, as far as small and medium-size dynamos are concerned. To put both machines into equal conditions as regards strains from centrifugal force, the speeds are assumed to be inversely as the linear dimensions. It is also assumed that both are worked up to the same safe limit of heating in the coils. From this it follows that the number of watts absorbed in heating the machines will be as $1 : q^2$. The above formulas for magnetic resistance show that the proportion of resistances will be $1 : \frac{1}{q}$, and that of exciting powers $1 : \sqrt{q^3}$. The fields will be as $1 : \sqrt{q^5}$, and the electromotive forces as $1 : \sqrt{q^3}$. The resistances of armatures will be as $1 : \frac{1}{q^2}$, and the current which may be allowed, so as to cause the same heating in both armatures, will be as $1 : q^2$. Let T be the total energy developed in the armature of the smaller machine, and W the energy wasted, the energy of the larger machine will be

$$T_1 = T\,q^{3\frac{1}{2}},$$

and that wasted will be

$$W_1 = W\,q^2.$$

The electrical efficiency of the larger machine is evidently

$$\mu = 1 - \frac{W}{T\,\sqrt{q^3}}.$$

This shows that the efficiency must increase with the size of the machine. On the other hand, if the size be reduced, a point will soon be reached where the efficiency is zero, and the machine has just sufficient energy to excite itself, but cannot do any external work; and if its dimensions be still further reduced it will cease to act. This will best be seen by an example. Suppose the case of a Gramme dynamo, the core of which is 6 inches in diameter, giving an electrical efficiency of 80 per cent. Here $\frac{W}{T} = 0 \cdot 2$. If a model of this machine be made one-third the size, with every turn of the wire and all other details of the construction faithfully copied, the efficiency of the model will be—

$$\mu = 1 - \frac{0 \cdot 2}{\sqrt{0 \cdot 333^3}} = 4 \text{ per cent.},$$

or 96 per cent. of the total energy developed in the armature of the model will be required to overcome its resistance, leaving only 4 per cent. for the external circuit. With an armature slightly smaller than 2 inches the model would not act as a dynamo at all. This explains why it is so difficult to make a small working model of a dynamo-machine.

The weights of the two machines compared above can be taken to be in proportion to the cube of their linear dimensions, but the cost of manufacture will have a somewhat lower ratio, since labour forms an important item which is not proportional to weight. Roughly speaking, the cost of labour is proportional to the square of the linear dimensions, and the total costs of the two machines can be taken to be in the ratio of 1 : $q^{2\frac{1}{2}}$. Since the output is proportional to $q^{3\frac{1}{2}}$, it follows that the cost of the dynamo per lamp is inversely as its linear dimensions. The following Table, calculated by the aid of the above proportions, will show that the figures obtained agree fairly well with those occurring in actual practice:—

Armature	10 inches	15 inches
Speed per minute	1,000 revolutions	670 revolutions
Number of lamps	150	620
Weight	10 cwt.	34 cwt.
Price	£100	£276
Price per lamp	13*s.* 4*d.*	8*s.* 11*d.*
Efficiency per cent. . . .	80	89

For armatures larger than 15 inches the above proportions are no longer applicable, and it will be more nearly correct to assume that the area of conductor on armature is as the square of the linear dimensions than as the third power, as was assumed in the case of small and medium-sized dynamos. The area is probably between q^2 and q^3, and in that case the output would be proportional to something between q^3 and $q^{3\frac{1}{2}}$. It will thus be seen that the advantage of large over small dynamos is most apparent where the thickness of insulation and the clearance form a considerable item of the total space available; but that in very large dynamos, where this item is of less comparative importance, the advantage is not so great.

Another point which influences the electrical output of dynamos, as compared with their weight and cost, is the electromotive force. It is evident that of two dynamos of equal linear dimensions, but wound with wire of different gauges, that will give the greater output which has the stouter wire, because the space wasted by insulation is less. On account of the great variety of conditions which might exist in each case, it is not possible to bring the relation of output to electromotive force into the shape of a formula generally applicable. But the influence of electromotive force can be shown by an example. Take the case of an armature wound with 0·20-inch square wire, giving 160 volts and 200 amperes. This would suffice for feeding sixty 10-ampere arc-lamps grouped in three parallel series, twenty in each. If a similar armature-core were wound with 0·05-inch wire, twelve times the number of turns could be got into the same space, bringing up the electromotive force to 1,920 volts. If both armatures are to be equally safe as regards heating, the current in the second case would be

10 amperes. The output of the high-tension machine would be 19,200 watts as against 32,000 watts of the low-tension machine of equal linear dimensions. Allowing 47 volts per lamp when all are grouped in series (as against 53 volts when grouped twenty parallel, the loss of 6 volts being occasioned by resistance placed in each parallel to enable the lamps to feed), the high-tension machine could light only forty-one lamps, whereas the low-tension machine can light sixty. From this example it will be evident that where a large number of arc-lamps is required within a limited area, as, for instance, in mills and factories, and where the length of conducting wires is comparatively small, it is more economical to group the lamps in parallel series, and to employ a low-tension dynamo. When lighting streets, docks, and extended areas the case is different, as then the cost of cables becomes so considerable, in comparison to that of the dynamo, that it is more economical to reduce the former at the expense of the latter by placing all the lamps in series.

III. PRACTICAL CONSIDERATIONS AND DESCRIPTION OF DYNAMOS GENERALLY IN USE AT THE PRESENT TIME.

In entering upon this part of the subject the Author is well aware of the difficulties which stand in the way of presenting anything like a complete report of the present state of the manufacture of dynamos, and of his inability to overcome them. If the following descriptions are not as complete as might be desired, and as members have a right to expect, he would ask their indulgence, on the plea that the subject is very large and still growing; that information about practical details and special types of dynamos is very difficult to get; and that, even if obtained, it could not be inserted without making this Paper too long. At the same time he would take this opportunity of expressing his indebtedness to those makers who have in a most liberal manner supplied information, and especially to Messrs. Mather and Platt, Paterson and Cooper, Elwell and Parker, Crompton, Greenwood and Batley, the Brush Company, and the Gülcher Company. Regarding some other very important dynamos, he has unfortunately not been able to obtain information, and it is to be hoped that these and other shortcomings may to some extent be made good during the discussion.

It has been mentioned in the beginning of this Paper that as regards the armature, all modern continuous-current dynamos can be ranged under three headings: the drum, the cylinder, and the disk. It will therefore be convenient not to classify dynamos by their armatures alone, which in many cases are identical, but also by the shape of the field-magnets, and to facilitate such classification the diagrams, Figs. 1 to 20, Plate 1, have been prepared. These diagrams show at a glance the peculiar character of each type of machine, and enable the Author to dispense with much lengthy description, which in many cases would have to be repeated. The type of armature is written below each diagram, and the maker's, or designer's name, is written above.

Figs. 1 and 2 represent two types manufactured by Messrs. Mather and Platt, one known as the "Edison-Hopkinson" dynamo, having a drum-armature and a single magnet; the other known as the "Manchester" dynamo with cylinder-armature and double magnets. The former is an improvement upon the original Edison machine, which had several magnets with long thin cores joined by a common yoke and pole-pieces. By substituting for these one stout and shorter magnet, Dr. Hopkinson has been able to considerably increase the strength of the field, and therefore the output of the machine, whilst at the same time, he has succeeded in reducing the length of wire necessary to excite the field. The armature is wound with stranded-wire, and its core is composed of charcoal-iron disks. The internal resistance of the armature is so low that the machine, although only shunt-wound, is almost self-regulating. The Manchester dynamo is a modification of the ordinary Gramme type. The bottom pole-piece is cast in one with the bedplate supporting the bearings, and the top pole-piece is supported on it by short circular wrought-iron magnet-cores over which the exciting coils are placed. Electrical data and results obtained with these machines are given in a Table below.

Fig. 3 shows the field-magnets employed by Messrs. Siemens. A number of square or rectangular bars are bent to the shape shown, then placed side by side, and bolted together at the yokes. A slight clearance between the bars greatly facilitates ventilation, and prevents heating of the pole-pieces by induction from the armature. The magnets used by Messrs. Crompton (Fig. 4) are very similar in shape, but consist of solid slabs of wrought-iron without any subdivision, and the polar-cavity is bored out of the solid metal. Mr. Maxim (Fig. 15) used magnets almost identical with those employed by Messrs. Siemens. A new departure has been made by Messrs. Elwell and Parker, who have applied four poles to a long cylinder-armature (Fig. 6). As far as the Author is aware, this and Mr. Andrews' dynamo (Fig. 9) are the only cylinder-machines with more than one pair of poles. It seems on theoretical grounds quite feasible to increase the output of a cylinder-armature by surrounding it with four poles in the same way as has been done with disk-armatures. But there is the great practical difficulty of obtaining sufficient polar-surface, and a low magnetic resistance of air-space. This difficulty is somewhat increased if the cylinder be of small diameter in comparison to its length ; and if a multipolar arrangement must be adopted, it would evidently be advantageous to employ a large and short cylinder. But in the Author's opinion, it is doubtful whether in cylinder-machines the multiplication of poles constitutes any marked improvement. If the diameter of the armature has to be increased in order to get room for the application of four poles, it may be as well also to increase its radial depth, and get all that is wanted by two stronger poles. The fact that the generality of makers have not adopted the four-pole system seems to prove that its advantages are doubtful. The dynamo usually manufactured by Messrs. Elwell and Parker has only two poles formed of cast-iron segments (Fig. 5), which are pinned to the magnet-cores by a large number of wrought-iron bolts. The object of this arrangement is to increase the magnetic conductivity between the core and the pole-piece. The magnet-cores consist of four wrought-iron slabs bolted together at the corners and forming a quadrangle. The exciting coils are wound direct on to the magnets, and not on separate formers as is customary in some other machines. The "Weston" dynamo and that known as the "Phœnix," manufactured by Messrs. Paterson and Cooper, have both magnets somewhat resembling Crompton's. But the Weston magnets are of circular, or elliptical section, and the Phœnix magnets are of square cross-section. The latter are also placed farther apart than seems necessary, thus giving the machine a somewhat straggling appearance.

Messrs. Goolden and Trotter have abandoned the type of field called by them "Temple-Bar magnets" Fig. 8, and have returned to the shape shown in Fig. 8a, which is identical with the original Gramme field. The dynamo designed by Mr. Jones (Fig. 10) might be described as an original Gramme machine with one-half the magnets omitted. As pointed out above, the adoption of a single magnet in lieu of double magnets results in a saving of exciting wire. The Author's first machine intended for a very low speed had magnets as shown in Fig. 11. But the expense of curved magnet-forgings, and the difficulty of winding the exciting coils, increase the price of the machine, and where lowness of cost is a consideration the type shown in Fig. 12 is now used. The magnet is a single horse-shoe, consisting of three straight slabs, each excited by a separate coil, so that there is practically no yoke or non-excited portion. To diminish the resistance of air-space, the polar-surfaces are increased by cast-iron extensions. This type is more particularly intended for dynamos requiring to be driven direct by moderate-speed engines. For belt-driven machines, which can be run at a higher speed and need not have so large an armature, the Author has adopted the type shown in Fig. 13. A special form of tubular cast-iron magnet is used in the Thomson-Houston dynamo, Fig. 16. This is a single horse-shoe, although of a somewhat unusual shape. The yoke is formed by a series of wrought-iron bars fastened to the flanges of the two short cylinders which form the cores, and the two exciting coils are placed over the cylinders, nearly filling the annular space between them and the ring of bars. Each polar-surface is the zone of a sphere, and the armature is also of spherical form. In disk-machines, as represented by the "Brush," arc and incandescent, and the Gülcher dynamos, the cores of the magnets are always arranged parallel to the spindle, Plate 1, Figs. 17 to 20. In the Brush arc-machine there are two single horse-shoes, the polar-faces being parallel to the plane of rotation. Like poles are not joined so as to surround the disk on its outer periphery. In the Brush incandescent and the Gülcher machines a number of double horse-shoes are set around the disk so as to form four, six, or even eight poles, according to the size of the machine. In the Gülcher dynamo each magnet limb is cut off square, where it faces the disk, and is joined with that on the other side by a cast-iron U-shaped pole-piece (Fig. 20). In the Brush dynamo the magnet-cores are set rather farther out from the axis, and are not completely severed into two separate limbs (Fig. 19). Part of the polar-cavity is thus formed in the bar itself, and the polar-surface is increased by a cast-iron pole-piece shrunk on (Fig. 18). The latter arrangement is evidently preferable to that used by Gülcher, because the magnetic resistance, occurring at the joints between the core and the pole-piece, is thereby somewhat reduced.

The armature employed in the Brush dynamo for glow-lighting is shown in Plate 2, Fig. 21. The core consists of a wrought-iron ring upon which is coiled charcoal-iron tape, No. 30, contact between successive convolutions being prevented by thin insulating-paper coiled with the iron. When the desired radial depth has been obtained, the internal ring and some of the inner convolutions of tape are slotted out radially to receive metal arms, by which the core is supported on the spindle. There are two sets of arms clamped together with the core between them. Since it is of the greatest importance to prevent the armature from shifting in the direction of the axis and so destroying the winding by rubbing against the pole-pieces, the hub is secured to the spindle by large lock-nuts, and grooves are turned in one of the bearings of the spindle, which has corresponding rings by which the spindle is held as in a thrust-block. The winding of the armature and the connections with the commutator are of the usual Gramme type; but in order to avoid the use of more than two brushes, additional connections are introduced between opposite coils of the same potential, a system now universally used in all dynamos with more than one pair of poles.

The armature of the Gülcher dynamos (Fig. 22) consists of a central web of malleable-iron of H-shaped cross-section and of thin iron-washers laid into the annular grooves of this central ring. The plates are insulated from each other by distance-pieces, and are perforated so as to admit air to the inner portions of the core. The latter is supported on a gun-metal ring with flat arms radiating from a central hub, by which it is keyed to the spindle. It will be obvious from the construction of the armature that all the lines, except those which enter the core at right-angles to the spindle, must pierce the broad surface of the washers, and thus heat them. To get over this difficulty the Gülcher Company has recently tried a core of cotton-covered iron wire, laid into a gun-metal envelope of circular cross-section, radial slots being cut in the envelope to prevent its heating. The Company has also tried another type of core, consisting of coiled hoop-iron, and this has proved somewhat better than the core composed of disks. As will be seen from the Table below, the latter requires, at 5,000-feet circumferential speed per minute, 51·8 inches of armature-conductor for every volt of electromotive force developed in the armature; the new hoop-iron core only needs 40 inches.

The core of the Elwell-Parker armature consists of iron wire wound over a frame of radial arms, which support it throughout its entire length. To preserve a circular shape, segments of wood are inserted between the arms, but these are removed after the core has been completed. The iron wire is drawn through an insulating-compound during the process of winding, and after every few layers a slight cut is taken over the surface to prevent it getting out of truth. The extremities of the supporting arms are tipped with fibre, in order to insulate the core from the body of the machine.

The conductor is wound over the armature-core in a single layer, covering the external surface of the core completely, and the internal surface as far as it is exposed between the radial arms. From this it will be seen that the driving-power is transmitted to the core, partly by friction on the outer edge of the arms, and partly by the pressure of their sides against the internal coils, and their friction against the core. The outer wires are also kept in place by friction, which is increased by the pressure of the usual binding-hoops.

Mr. Crompton uses, for the core of his armature, charcoal-iron disks (about twenty-five to the inch), with notches on the inside, into which are dovetailed radial strips of phosphor-bronze, extending throughout the entire length of the core. These strips serve the double purpose of holding the core together lengthways, and of transmitting to it the driving-power from the spindle, into which their inner edges are fitted (Fig. 23). To avoid metallic continuity on the outside of the core, every alternate disk is coated with insulating paint, and the core is further subdivided lengthways by distance pieces of fibre, so as to allow air to pass through. A few of the disks, placed at equal intervals, are provided with projecting teeth, similar to the core originally used by Professor Pacinotti, and these teeth serve to transmit the driving-power from the core to the external wires. In Messrs. Paterson and Cooper's armature all the disks are provided with Pacinotti projections, and the conductor is laid into the grooves thus formed (Fig. 24). The core is supported by six or more insulated bolts passing through it parallel to the spindle, the ends of these bolts being fitted into gun-metal wheels, which are keyed to the spindle. On theoretical grounds a core of the Pacinotti type has a great advantage over the plain cylindrical form, inasmuch as by causing the projections to approach the polar surfaces with just sufficient clearance to allow rotation, the magnetic resistance of the air-space can be reduced almost to nothing. But in practice such perfection is unattainable. It is found that with a diminished clearance between projections and polar-surfaces the magnetic induction exerted by the former upon the latter becomes so great as to absorb a considerable amount of power, and to heat the iron of the pole-pieces by local currents. No amount of subdivision, such as could practically be applied to the metal of the pole-pieces, can entirely overcome this difficulty, and for this reason it must be admitted that these Pacinotti projections are after all not so great an improvement as theoretically they appear to be. It must also be noted that even if the clearance be made tolerably large there is no certainty of having entirely got rid of this loss of power, for there are no ready means by which it can be measured. The so-called electrical efficiency of the dynamo is in no way affected by any losses of this nature, and nothing short of a good transmission-dynamometer will serve to determine them. The Author would take this opportunity to protest against the present tendency of gauging the practical value and efficiency of dynamo-machines by their electrical efficiency. The two are only in so far connected as a low electrical efficiency always entails considerable loss of mechanical power; but on the other hand a very high electrical efficiency does not invariably mean a very small loss of power. Often the contrary is the case, especially when high electrical efficiency is attained by use of an excessive speed. It will be evident that losses of a similar nature to that above described, and which might be generally termed as magnetic friction, increase with the square or even the cube of the speed. They have absolutely no influence on the electrical efficiency, and yet power must be provided to make up for them and leave a margin for useful work. Thus it may very well happen, that a dynamo running at a high speed and giving 95 per cent. electrical efficiency is less economical than a low-speed dynamo giving only 85 per cent. electrical efficiency. In the Author's opinion the only correct way of gauging the value of dynamos is by their commercial efficiency, or ratio of power supplied to power given out.

The core of the Author's armature consists of soft charcoal iron-wire coiled on to a gun-metal supporting-cylinder (Figs. 25 and 26). The latter has end-flanges, and is subdivided lengthways by pairs of flanges, between which air can circulate. Pegs are cast on these flanges, which are insulated with fibre-ferrules, and project through the external winding. These projections or " driving horns " serve to transmit the power to the external wires, which do not completely cover the surface of the core, but are laid on in groups, which are kept apart by the driving horns and by additional distance-pieces, thus allowing air to pass freely through the core. The supporting-cylinder is provided with perforations corresponding with the spaces between the flanges and with internal circular ribs, in order to keep the internal wires a short distance off its surface, and thus admit air also to the back of them. The supporting-cylinder is mounted on the spindle by two sets of gun-metal arms, which are fitted into keyways at each end. For the purpose of winding, the arms are removed and short wooden dummy-spokes are inserted in their stead, so as to leave the whole of the interior accessible. When the winding is completed the arms are forced in, and, since they extend only a short distance inward at either end, the spaces between them, which have been left free from the winding, serve to admit air to the inner surface of the supporting-cylinder.

Of high-tension dynamos, constructed exclusively for arc-lighting, only two types of importance are in use, although all the machines above described can be wound for, and are often used for, arc-lighting. These special types are the Brush and the Thomson-Houston. The former is so well known that a detailed description seems superfluous; but it should be mentioned that through the substitution of the new wrought-iron armature core for the old cast-iron core, the electromotive force of the machine has been increased by about 40 per cent. The Thomson-Houston dynamo is comparatively little known in this country. Its armature is a modification of the Hefner-Alteneck drum, but of spherical form. There are only three coils on it, connected to a three-part commutator, whilst the inner ends of these coils are connected together. Two very wide brushes—or, to speak more correctly, their equivalent, namely, two pairs of brushes, each pair spanning a considerable arc on the commutator—are employed, and in consequence of this arrangement each coil is in turn connected in series with the two others which are parallel. It will be evident that, on account of the small number of coils, the self-induction in them must be very great, and must cause severe sparking at the brushes. To overcome this difficulty a special rotary-blower is fixed to the spindle, and the air is delivered by two nozzles to the commutator. The blower is set so as give a puff of wind each time a segment leaves its brush, thus blowing out the spark. It is probable that this air-blast is essential to the safety of the machine. The interaction between the coils of the armature, between the latter and the field, and also the peculiar system of automatic regulation whereby the current is kept constant under a varying speed or external resistance, are all exceedingly interesting, but time will not permit entering into these matters at length.

By the courtesy of several of the manufacturers of dynamo-machines the Author has been able to collect some data, which he has arranged in tabular form. The great difficulty in compiling a Table of this description is to find a common basis for comparing machines which are of all possible sizes and types. In order not to make the comparison unfair to the smaller machines, it was necessary to omit all those points which depend more or less directly on the size of the machine. Thus it was necessary to leave out the efficiency, both commercial and electrical, as also the total weight and the number of watts produced for every lb. of copper used in the construction of the machine, although these points are of great practical interest. The Table is not so complete as might be wished, and many of the figures have not been obtained direct from the makers, but had to be calculated from data given by them. It should be remembered that the numbers refer only to one particular dynamo of each maker, and need therefore not necessarily be representative numbers for all his machines. As regards the length of conductor on the armature per volt of electromotive force, some doubt might possibly be entertained whether it be correct in all cases to determine the figure by simply dividing the total length of wire by the internal electromotive force. This would, in the Author's opinion, be unfair to multi-polar-machines, because in so doing, no account would be taken of the capacity of the armature to give an increased current. He would suggest that the ratio between the length of wire and the electromotive force be in all cases divided by the number of pairs of poles, and he has added these figures, which are bracketed, to the simple ratio.

A careful perusal of the Table (p. 151) will show that generally speaking the construction of dynamo-machines has not yet become an exact science. There is so much difference in the figures relating to the same thing in different machines, that one is inclined to think the proportions adopted by some makers are rather the result of tradition or accident than of careful calculation. On the other hand it must be remembered that the manufacture of dynamos is of very recent origin, and that with the great improvements which are continuously being made, there has hardly yet been time for any one type to settle into final proportions.

IV. Engines used for driving Dynamo-Machines.

The improvements in dynamo-machines within the last few years have necessarily brought about a complete change in the methods of driving them. In its earlier days the dynamo required a high speed, and absorbed comparatively little power. Where an engine was already at hand doing other work, it could often be used to drive a small dynamo, in addition to its other load, and for that purpose a counter-shaft was generally required to get up the necessary speed. Where no engine was at hand, the usual thing was to put down a portable, or semi-portable engine, driving one or more dynamos by means of a counter-shaft. Regularity of speed was not a matter of paramount importance, as the dynamos were

Name.	Type of Armature.	Normal Number of Revolutions per Minute.	External.		Attachment of		Ventilation.			Density of Current. Amperes per Square Inch.			Number of Lines per Sq. Inch.		Length of Armature-Conductor per Volt in Inches.			Cooling Surface. Square Ins. per Watt transformed into Heat.	
			Pressure.	Current.	Core to Spindle.	Wire to Core.	External wires, E.	Internal wires, I.	Interior of core, C.	Armature.	Main.	Shunt.	Armature-core.	Magnet-core.	At Normal Number of Revolutions.	At 3,000 Feet per Minute.	At 5,000 Feet per Minute.	Armature.	Magnets.
Edison-Hopkinson	Drum .	800	110	300	..	Friction	E.	..	..	2,570	..	..	..	10·3	24·8	18·2	..	0·90	..
Manchester . .	Cylinder	1,050	110	200	..	,,	E.	I.	..	3,200	2,060	1,750	17·0	10·5	31·6	34·8	..	0·55	0·76
Crompton . .	,,	410	110	230	Dovetail bars	Toothed disks at intervals	E.	I.	C.	2,200	..	..	24·0	12·8	52·5	26·2	..	1·40	..
Elwell-Parker .	,,	1,600	60	65	Metal arms by friction .	Friction	E.	I.	..	6,150	..	..	13·6	9·7	54·6	53·2	..	0·22	..
Elwell-Parker, 4-pole . . .	Long cylinder	400	110	160	,,	,,	E.	I.	..	3,300	..	580	13·0	11·7	119·2 (59·6)	72·0 (30·0)	..	0·28	8·00
Paterson-Cooper	Short cylinder	500	110	380	Insulated bolts and metal arms	Grooves in core	E.	I.	..	2,100	900	800	19·5	8·8	42·5	41·6	..	0·61	1·90
Jones	,,	950	100	64	Metal arms by friction .	Friction	E.	I.	..	2,100	1,600	500	11·9	11·1	83·0	103·0	..	1·48	3·15
Andrews . . .	,,	500	110	110	Wooden hub by friction	,,	E.	..	..	1,300	1,300	..	16·0	..	71·5	55·0	..	1·06	..
Goolden-Trotter .	,,	950	60	200	Metal arms let in	,,	E.	I.	..	295	1,180	630	13·4	12·1	44·0	41·5	..	1·80	2·68
Kapp	Cylinder	680	110	125	,,	Driving horns	E.	I.	C.	2,720	1,000	1,180	25·0	12·4	49·2	35·0	..	1·23	1·50
Brush, 4-pole .	Disk .	800	70	150	,,	Friction	E.	I.	..	1,800	1,920	800	17·5	12·4	65·2 (32·6)	..	57·5 (28·7)	2·30	1·60
Brush, arc . .	,,	650	2,700	10	Metal arms bolted to core	Projecting teeth	E.	I.	..	930	710	..	..	..	53·5	..	48·5	0·90	2·00
Thomson-Houston, arc	Sphere.	850	1,600	9·6	Cast-iron shell	Friction	E.	..	..	1,750	750	..	..	..	76·0	128·0	74·5	1·72	..
Gülcher, 8-pole .	Disk .	400	65	400	Metal arms cast with rim	Projecting teeth	E.	I.	C.	2,500	1,250	..	10·1	6·6	296·0 (74·0)	..	207·0 (51·8)	1·00	1·60

then exclusively used for arc-lighting, and in those days the light of arc-lamps was equally flickering whether the speed was absolutely constant or not. As lamps improved, and as the glow-lamp came into use, the necessity of a constant speed became apparent, and some engine-makers began to build special so-called "electric-light engines," the speciality for the most part consisting in the substitution of a good sensitive governor for a bad one to an ordinary engine. By degrees these special engines were built stronger, and arranged to run at a higher speed, thus doing away with the necessity of using a counter-shaft. An excellent example of this class of engine is the Armington-Sims, now being manufactured in this country by Messrs. Greenwood and Batley. It is an open double-acting engine, and has a single piston-valve, worked by what might be termed a compound-eccentric, which is movable on the shaft, but the position of which is controlled by the heavy weights of a centrifugal-governor acting against spiral springs. The governor is mounted within a pulley on the engine-shaft, and its weights are connected by two links to ears on an inner eccentric, and by a third link to an eccentric-sheave mounted upon the latter. The connections are arranged in such a way that the inner and outer eccentrics are shifted in an opposite sense, thus causing the centre of the outer eccentric, to which the usual straps and rod are fitted, to be shifted practically in a straight line. In consequence of this arrangement the lead of the valve is kept nearly constant for all points of cut-off.

Another engine belonging to this class is the "Manchester" diagonal-engine, manufactured by Messrs. Mather and Platt. It has two cylinders, inclined at right-angles to each other, and working on to the same crank. The governor is also mounted on the engine-shaft and controls the cut-off. A great advantage of this type of governor over that driven by a belt, is that racing of the engine in consequence of the belt breaking is impossible. To minimise the risk of such an accident, where a governor of the usual type is used, motion should be given to it always by two distinct belts, or, better still, an independent electric-governor should be added.

By far the most important engines for driving dynamo-machines are those coupled direct to the dynamo-spindle. The importance of these engines will be apparent when it is considered that the present tendency is to build ever larger dynamos, and that even if there were no mechanical difficulties in the transmission of large powers by belting, the necessity of compressing electric light machinery into a minimum of space, would alone be sufficient to force on the adoption of the direct-acting type of engine. The engines now available for this purpose may be classified under three headings: 1. The rotary-engine, as represented by the "Tower," the "Fielding and Platt," and others in which rotary-motion is produced without the intervention of connecting-rods. 2. The single-acting reciprocating closed-engine, as represented by the Brotherhood, the Willans, the Westinghouse, the Goodfellow, Matthews, the Elwell-Parker, and other engines, in which the usual parts, viz., a straight cylinder, piston, connecting-rod, and crank are employed, but where steam is admitted only to one side of the piston, and where all the working parts are closed in by a tight casing. 3. The ordinary open double-acting engine. It is not surprising that the demand for high-speed engines, created by the introduction of electric-lighting, should have stimulated inventors to renewed exertions with regard to rotary-engines, which, justly or not, have at all events the reputation of being particularly suitable for high speeds. But it is not high speed alone which qualifies an engine for electric-light work; and there is the danger that in aiming to attain perfection in this direction, serious faults may be developed in other directions. The three great faults in all rotary-engines are want of economy, frequency of breakdown, and difficulty of repair by ordinary workmen and with common tools. The closed single-acting engines are not intended for equally high speeds, but, on the other hand, they are as a rule more economical and durable, though in this respect there is much room for improvement, as shown by the frequent failures of engines. The present tendency of using high-speed closed-engines for driving dynamo-machines is partly the result of precedent, and partly due to the desire of keeping the size and cost of the dynamo as low as possible. Some years ago, when low-speed dynamos were unknown, no engine of the double-acting type could be found which would work dynamos direct, and the few engines which were then tried failed, simply because the speed was excessive. Since then, however, the speed of dynamos has been reduced more and more, and is still being reduced, thus lessening the one great difficulty which stands in the way of the employment of a good substantial double-acting engine, whilst at the same time the increase in the size of dynamos, and of the power required, renders the question of economy of vastly greater importance than it was a few years ago. Apparently there is no reason why electricians should be content with less economical engines than ship-owners or mill-owners, provided they can reduce the speed of dynamos, and bring it within reasonable limits. No marine engineer would think of using a rotary- or other closed-engine for a vessel, even if only a moderate amount of power were required; and if it were not for the force of precedent, and the difficulty of high speeds, electricians would be of the same opinion with regard to dynamo-machines. But the difficulty of speed is not a real one; it is always possible to reduce the speed to any desired degree by making the armature sufficiently large, and in so doing the commercial efficiency of the machine need not be reduced if all the parts be correctly proportioned. These considerations have induced Messrs. W. H. Allen and Co., who are associated with the Author in the manufacture of dynamos, to adopt the system of driving direct by open, double-acting twin-engines. Figs. 27 and 28 show one of the arrangements employed, which has been at work during the recent International Inventions Exhibition at South Kensington. The speed is fixed at 340 revolutions per minute, and this limit has, during the five month's working, proved perfectly practical. The dynamo was designed for two hundred lamps, but nearly the whole of the time, it was used to feed two hundred and eighty lamps at the end of a long circuit, which was equivalent to about three hundred lamps at the terminals, or an excess of 50 per cent. over its regular load. Thus the two engines indicated about 35 I.HP. at the speed mentioned. The cylinders are 6½ inches in diameter with 5 inches length of stroke, and the cranks are set at opposite angles to obtain a perfect balance, and to reduce the pressure on the bearings. The piston-speed of this engine is only 283 feet per minute, and even if it were higher the Author would not hesitate to use it. He learns from Messrs. Greenwood and Batley that the Armington-Sims engine will run perfectly steady at what some engineers would consider an excessive piston-speed. One of these engines, 9 inches in diameter by 12 inches length of stroke, driving fifty-six Brush arc-lamps, has been running in Boston for some time, during thirteen hours a day, at a speed of 310 revolutions per minute. According to the number of lamps the indicated power must be about 50 I.HP., and the piston-speed is 620 feet per minute. These examples will show that it is practically possible to build open double-acting engines which shall satisfy the requirements of electric-lighting, provided the demands as to speed are reasonable. Instead of trying to design engines which shall run fast enough for high-speed dynamos, engineers must reduce the speed of the dynamo so as to suit the engine.

The Paper is accompanied by numerous small-scale drawings, from which Plates 1 and 2 have been engraved.

[DISCUSSION.

Discussion.

Sir Frederick Bramwell. Sir FREDERICK BRAMWELL, President, in proposing votes of thanks to Mr. Imray and Mr. Kapp, said that the discussion on the two Papers had better be divided. First, the subject of motors would be taken, which might be divided into rotary-engines, single-acting reciprocating-engines, and ordinary double-acting engines. The next subject would be that of dynamos, under the three heads of the Hefner-Alteneck principle, the Gramme principle, and the Schuckert principle. Those matters having been treated of, what might be called the general discussion could then be entered on. He did not suggest that a speaker, when referring to the rotary-engine, should not compare other engines with it, or that he should refrain from touching on the question of dynamos, the object being as far as possible to make the discussion systematic; but that such system should be so far departed from as to obviate the necessity of any speaker rising a second time.

Mr. Imray. Mr. IMRAY referred to the models of the engines mentioned in his Paper. The steam-turbine was one that he made a few years ago, after the fashion of Mr. Froude's liquid brake. There were a number of ratchet-cells in the circumference of the wheel, each of these cells being semicircular in section, and there were corresponding cells in the shell. The steam entered and was supposed to play in a sort of vortex into one cell of the wheel, out of that into the wheel-casing, and so on, following a vortical movement all round until it issued at the outlet. The machine exhibited was what was actually driven. The usual speed was 12,600 revolutions per minute, but 12,000 was about the best. There was attached to it a friction-brake carrying a weight, so that the power could be calculated. He measured the quantity of steam exactly, and found that it was not economical, using twice as much steam as an ordinary high-pressure-engine. Another specimen of rotary-engine was Mr. Tower's ingenious machine. It was a universal joint enclosed in a spherical shell, with expanding and contracting cavities. In Mr. Fielding's engine there was also a sort of universal joint, four pistons working in four segments of bent pipe (they could not be called cylinders) upon curved axes. One advantage claimed for it was that the packing was of the ordinary spring-ring kind. He also exhibited one of the engines used in the fish-torpedo by Mr. Brotherhood. It was worked by compressed air. There was also a model of Mr. Brotherhood's com-

Mr. Imray. pound-engine, in which there were three cylinders, each of them being doubled.

Mr. Kapp. Mr. GISBERT KAPP directed attention to the diagrams illustrative of his Paper, showing that different kinds of field-magnets had been drawn for the same diameter of armature, so as to give an approximate idea of the difference in bulk and weight between the different types of machines. The idea was only approximate, because the same-sized armature in one type might be much more efficient than in another type. Amongst the diagrams there were three different kinds of magnet, which were fairly representative of three types of dynamos used by him under certain conditions. Plate 1, Fig. 11, was intended for dynamos which went slowly, and must not exceed a certain weight, so that there was not much iron, and what was used was in a position to contribute to the magnetic-field. There was no side and unexcited portion, and no yoke on the magnet. That type was rather expensive and somewhat troublesome to wind. Special tools were required for the purpose, but with those tools there was not much difficulty. Fig. 12 was a cheaper type, but somewhat heavier, because in the corners a certain amount of iron was not directly productive of lines of force. On the other hand, the amount of copper wire required for the single magnet was considerably less than for the double magnet. Fig. 13 was a still cheaper type. The magnets were cylindrical, and could therefore be turned in a lathe, which was always cheaper than planing. Only two magnets were used instead of three, so that the expense of winding was somewhat less. He desired to draw attention to one point which could not yet be well brought within the grasp of theory, but was nevertheless an important matter, namely, the enormous difference sometimes occurring in dynamo-machines according to the iron which was used. It was almost impossible to foretell accurately the performance of a machine unless it was quite certain that the sample of iron submitted for the magnet was exactly the same as that actually used in the magnet. He had known a case where two machines had been made of exactly the same dimensions and treated in the same manner, and one had given 20 per cent. more electromotive force than the other. He had inquired of the makers whether they annealed the first magnets, and the reply was that they thought they had done so, but as they had only three days in which to execute the work, perhaps they had not annealed them enough. Another pair was then ordered, two weeks being given for the work, and the machine turned out 20 per cent. more electromotive force. When manufacturers had to deal with so subtle a thing as the magnetic properties of iron which they did not at present quite understand, it was very important to minimize the chance of failure by giving a very long time for annealing. It was no use to hurry those who had to make the forgings, because in that case the magnets would certainly be hard and unsatisfactory. Mr. Kapp.

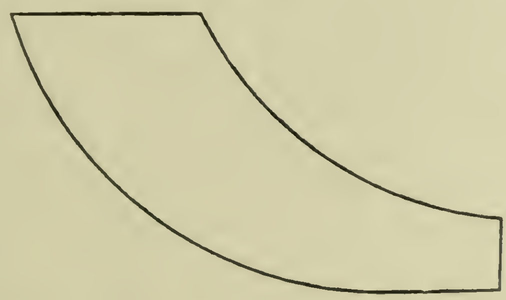
Mr. Tower. Mr. Beauchamp Tower observed that Mr. Imray had very lucidly explained the advantage derived by a reciprocating-engine from the inertia of its reciprocating parts, which tended to equalize the irregular force of the expanding steam; and he had also pointed out that all rotary-engines must in some sense be reciprocating-engines. This was certainly true of the spherical-engine, and in so far as it was a reciprocating-engine it derived exactly the same kind of advantage from reciprocation as other engines. It possessed, however, a quality which he thought gave it a decided advantage over all reciprocating-engines for use at high speeds with which he was acquainted. It was absolutely and perfectly balanced, so that no matter at what speed it ran, all the forces necessary for the acceleration and retardation of its parts were perfectly contained within itself, and were not imparted to surrounding objects. It could consequently be run at very high speeds without requiring foundations, and without imparting vibration to anything outside it. This, he believed, was more than could be said of any reciprocating-engine at present. All the methods in use for balancing reciprocating-engines were only very rough approximations. The common way was to attach weights to the backs of the cranks, so that when the piston was moving in one direction, the balance-weight was moving in the opposite direction. But these balance-weights moved in a circular path, whereas the parts they were intended to balance moved in a straight line. The consequence was that at mid-stroke the balance-weights were exerting an unbalanced centrifugal force at right-angles to the line of motion of the piston. These weights were therefore not usually made to more than about half balance the reciprocating parts, and the general result of this mode of balancing was to halve the unbalanced strains, and to cause them to recur four times in the revolution instead of twice. The Author of the first Paper described an engine having two vertical single-acting cylinders, having their pistons connected to opposite cranks on a shaft below them, and said, "In this case the reciprocating-weights are completely balanced." He would just go into a few figures to show how really incomplete their balance was. He would assume that each of the cylinders was 6 inches

Mr. Tower. in diameter, and had a length of stroke of 6 inches, and that each piston with its connecting-rod weighed 30 lbs.; and also that the connecting-rods were three times the length of the crank-radius, and the engine was run at 1,000 revolutions per minute. He would use the Author's diagram, Fig. 2, p. 116, to calculate the forces of reciprocation at the ends of the strokes of the pistons. Each piston and connecting-rod weighed 30 lbs. The crank-radius was 3 inches or $\frac{1}{4}$ foot. The revolutions were 16·66 per second. Then $30 \times \frac{1}{4} \times 16{\cdot}66^2 = 2{,}078$, being the coefficient by which to multiply the ordinates of the diagram in order to ascertain the reciprocating-force. He only wanted them at the two extreme ends of the stroke. The ordinates were 1·64 at the end of the stroke nearest the shaft, and 0·82 for the other end. As it was supposed to be a vertical inverted engine, he would call the former position the bottom of the stroke, and the latter the top of the stroke. $1{\cdot}64 \times 2{,}078 = 3{,}408$ lbs. = the downward force at the bottom of the stroke: $0{\cdot}82 \times 2{,}078 = 1{,}704$ = the upward force at the top of the stroke; so that when the piston at the bottom of its stroke was pushing downwards with a force of 3,408 lbs., the piston at the top of its stroke, which was supposed to balance it, was only pushing upwards with a force o 1,704 lbs. In this engine, therefore, there was an unbalanced force of upwards of $\frac{3}{4}$ ton put on and taken off its foundations two thousand times in a minute. But besides this, the two cylinders were not in one line, so that in addition to the above there was a moment of 1,704 lbs. on the end of a lever, which could not be taken as less than 7 inches, acting first one way and then the other one thousand times in a minute, tending to rock the engine in the plane of the cylinders and shaft. He thought, therefore, that the engine under these circumstances would impart vibration to surrounding objects, and would require to be pretty securely fastened down. Of course he was aware that it was possible to construct a perfectly-balanced reciprocating-engine; but he contended that it could only be done by a considerable addition to its complication and number of working parts; whereas in the spherical-engine perfect balance was inherent in the principle of its construction. He believed he did not overrate the importance of this perfection of balance for high speeds such as 1,000 revolutions per minute and above. It was, of course, comparatively unimportant at moderate speeds, and the ordinary approximate balance was then usually sufficient; but when it was considered that the forces increased as the square of the speed, that whatever force was due to imperfect balance in an engine

Mr. Tower.

running at 100 revolutions became multiplied a hundred fold when that engine was run at 1,000 revolutions in a similar time, and also that the rapidity of recurrence of the impulses at high speeds was much more likely to synchronize with the natural period of vibration of walls and floors of buildings and other structures, and so to produce a greatly aggravated vibration of those structures according to the well-known law of induced vibrations, he thought that perfect balance was important, and that an engine which naturally possessed it was, other things being equal, more suitable for high speeds than one that did not. Mr. Imray had pointed out the way in which the forces of acceleration and retardation acted in combination with the steam during the out-stroke. But he had not referred to what occurred in the back or exhaust-stroke. When the piston was moving back just the same force was required to accelerate it and to retard it, but the steam was not there acting in the same way as on the steam-stroke, and the consequence was that in a single-acting engine it was necessary to cut off the exhaust earlier than was generally done in ordinary engines, and to create an unusual amount of cushioning in order to stop the piston. When the piston had got to the middle of its stroke it became necessary to put a force to resist its further entrance into the cylinder; and to produce that force it was necessary to cushion the steam. That gave a diagram something like Fig. 4. But he did not know that there was any very great harm in it if the clearance was properly arranged, and so proportioned that the steam would not be compressed too much. Mr. Willans provided for always keeping the connecting-rod in compression, by having a piston on the top of the working piston of his engine with a constant pressure of steam. The spherical-engine was supposed to be enclosed in a hollow sphere; and the edges were packed so as to be steam-tight against the inner surface of the sphere. The sphere was first divided into halves by a central disk, and each of those halves was subdivided by pieces

Fig. 4.



Mr. Tower. called blades jointed with a sort of hinge on to the disk, so that the inside of the sphere was divided into four compartments. The hinges of the blades were at right-angles to one another. It would be understood that when one compartment was fully opened, the angle between the faces was 90°, so that the space which was enclosed between the outside of the sphere and the two faces was one-fourth of the whole sphere. As it turned round it gradually closed until it got to the bottom, when it was completely closed. The one opposite, which was before closed, was then opened. The same thing happened on the other side. The steam-ports were so arranged that steam was let in when the compartments were opening and pressed the disk and blade apart, and pushed them round; then it came in at the next compartment and pushed that apart, and so on. The compartments were open to the exhaust when they were closing. Valves which were in the base of the blades were arranged that the steam was cut off at any point that might be desired, so as to get expansion. The capacity swept through in one revolution was nearly equal to the whole capacity of the sphere, so that if the engine was used as a pump the quantity of water pumped would be the whole sphere full, subject to a small reduction by the displacement of the hinge-joints—a reduction of about 10 per cent.; therefore in actual practice the capacity swept through was nine-tenths of the whole capacity of the sphere. Thus the space occupied by the engine was reduced to the space occupied by a single-acting cylinder, and he thought that there could not be any greater reduction than that. The only essential part of the engine was the sphere and its internal moving part, and the power given out by a revolution was equal to that of an engine with a single-acting cylinder having the same capacity as the capacity of the sphere.

Mr. Fielding. Mr. J. Fielding said that he could not quite agree with Mr. Imray's first conclusion, but as Mr. Tower had fully gone into that subject he need not refer to it further. With the second conclusion he could to some extent agree, and the third had his entire approval. The justness of the first proposition to some extent depended upon how far the second might be considered established, since the question of reciprocating- versus rotary-engines was one largely as to whether reciprocation was or was not an evil. It was admitted by inference, in the second proposition, that reciprocation might be an evil, and a remedy was suggested. He would, however, submit that the necessity for such remedy, to say the least, pointed to the fact that reciprocating-action was detrimental in high-speed engines, and thus to

Mr. Fielding.

some degree contradicted the first proposition. He would point out also that, under certain very probable conditions, the remedy suggested might only serve to intensify the original defect, and at all events could only be considered to be a remedy so long as the engine ran at one particular speed, with a fixed degree of expansion and initial pressure of steam. Suppose the speed to be suddenly increased, an earlier cut-off, or a reduction in the steam-pressure, to take place, it was conceivable that the acquired momentum of heavy reciprocating-parts would tend to cause, rather than prevent, knocking in the bearings. In a single-acting engine in such a case the tendency would be to cause the bearing-surfaces to separate, and thus the principal advantage claimed for this type would disappear. With reference to the equalizing effect of heavy reciprocating-parts, it was a question whether this was altogether an advantage. What was wanted was a regular tangential effort, not that a regular effort should be exerted upon a reciprocating-piston. As showing that the two things did not go together, he would point out that in the case of a pair of engines coupled at right-angles, having full steam on throughout the stroke, the variation in tangential effort was about 20 per cent., whilst in the case of an engine cutting off at half-stroke this variation amounted only to 14 per cent. It was to be hoped that some of those who had had more experience in the single-acting reciprocating-engines, would be able to state some facts bearing upon the employment of heavy reciprocating-parts in such engines. The general impression, he thought, was that makers of that class of motor made the reciprocating-parts as light as was consistent with strength, and area of the bearing-surfaces. Mr. Imray suggested the use of very short connecting-rods as a further means of equalizing the effort on crank-pins; but although he showed the effect of using a rod of only one-and-a-half stroke, he did not gather that he advocated in practice such very short rods. In single-acting engines in which the piston acted as a cross-head, and the cylinder as a guide, even with a rod of, say, two-and-a-half strokes, it would be admitted that the action of the connecting-rod was by no means a beneficial one, and certainly the evil would be largely increased by such very short rods. Notwithstanding his first conclusion, Mr. Imray dwelt at some length upon the inherent defects of reciprocating-engines as compared with rotary-engines. One of these defects was undoubtedly the difficulty of balancing, and certainly, at speeds of 1,000 and 1,500 revolutions per minute, this difficulty became a serious one. It was almost impossible

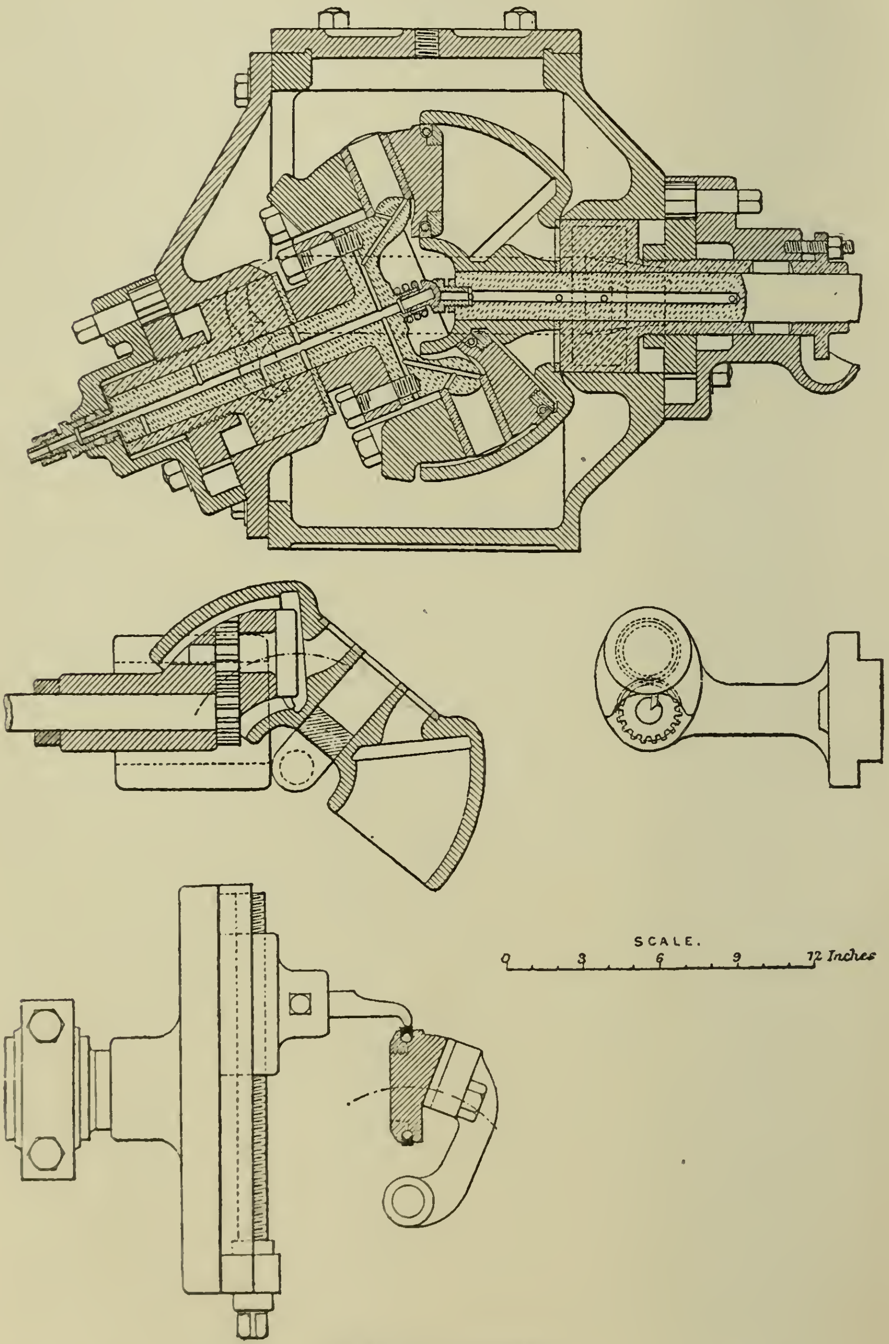
Mr. Fielding. to balance a reciprocating-engine perfectly, and at the same time to obtain even an approximate approach to regular tangential effort. Three-cylinder engines were suggested by Mr. Imray as probably meeting the requirements of the case, and possibly in the latter respect they were very effective at moderate speeds; but as for balancing a three-cylinder engine, he thought it would be admitted to be impossible. With reference to the type of engine in which he was more particularly interested, aptly termed by Mr. Imray "reciprocating-rotary," it would be admitted, probably, that at all events theoretically this type combined in a high degree the best points of the purely reciprocating and purely rotary types of engine. Like the former, their motion was the result of the alternate enlargement and reduction, under steam-pressure, of closed spaces. The distribution of steam, also, was under full and positive control, so that admission, cut-off, expansion, release, and compression might be determined in any desired proportions. At the same time they involved very little motion that was not purely rotary, such as there was being capable of perfect balance, and acting in a direction approximately parallel with the driving-shaft, so that it might be practically disregarded. He would submit, therefore, that theoretically this class of motor did present some special advantages as compared with reciprocating-engines for high-speed purposes. How far it was possible to secure these advantages in a practical way, the examples before the meeting would to some extent show. Engineers not so long ago were very apt to condemn, without examination, anything of the nature of a rotary-engine, but latterly they had been more disposed to give a careful consideration to engines of this class. This change of opinion probably might be traced to the fact that such engines did possess some important advantages as high-speed motors, and that they were especially suitable for the direct-driving of dynamos, fans, &c. He thought that, other things being equal, engineers would rather do without reciprocating-action if possible. Engines of the Tower type had the advantages of occupying exceedingly little space, and they weighed less than any reciprocating-engine of equal power. These in themselves were very great advantages in numerous instances, such as for use on shipboard. They had few working parts, and these were capable of being perfectly balanced. The main arguments brought against them were that they were wasteful of steam, and that some of the details, being of a very special character, required for their production special tools, thus making repairs difficult where such tools were not available. With reference to

Mr. Fielding.

the engine with which he was more particularly interested, he thought it would be admitted that these two main objections had been largely overcome, since the piston-packings were practically identical with those used in ordinary reciprocating-engines. He submitted a sample piston and ring for examination. As to the steam-consumption of the Fielding engine, he regretted that at this moment he could not place definite results before the meeting. This was to a great extent due to the short time which had elapsed since he began operations with the engine, it being but about ten months since he commenced the drawings of the first engine; that was to say, with any intention of constructing an engine. Before the year was completed, however, he expected to be able to produce definite data upon this head. Steam-consumption, of course, was to a great degree a question of the steam-tightness of pistons and valve-faces, and as to the qualities of the Fielding engine in these respects he had positive information. With an engine held by a brake, at a standstill, and with full steam turned on, there was practically no escape of steam at the exhaust-pipe—certainly none that could be heard, scarcely any that could be seen; and, listening to the sound of the exhaust-pipe at low speeds, the beat could not be distinguished from that of a reciprocating-engine, being sharp and clear. With particular reference to the two examples of the reciprocating-rotary type of engine now before the meeting, it might be as well to say a few words as to their similarity. Though at first it might be contended that the Tower and the Fielding engines were identical in principle, a close examination brought out some very important differences, and made it evident that whilst geometrically similar, they might be said to belong to distinct classes. Clearly the former was a disk-engine. Like the Bishop engine, it had a disk- or diaphragm-piston working within a spherical casing, with, however, a different method of connecting the two shafts to each other and to the piston. This difference was very important, and made the Tower much superior to the Bishop engine. On the other hand, it could not be said that the "Fielding" was a disk-engine, since it had neither disk nor spherical casing. A casing of any kind was not essential to its working, but was simply a convenience. In this engine, a gimbal-ring and four pistons were substituted for the disk, and the equivalents of four cylinders took the place of the spherical casing. Some of the results of this construction were that the working parts were readily accessible for examination and adjustment, and might be exposed to view whilst in motion. What, however, was of greater

Mr. Fielding. importance was that the very high velocity of the piston-surfaces was avoided, this velocity being reduced to about one-third of

FIGS. 5.



FIELDING ENGINE.

that of the disk-piston. The peripheral velocity of an 8-inch disk-piston at 1,000 revolutions amounted to more than 2,000 feet

Mr. Fielding.

per minute, in addition to its motion in a transverse direction, whilst the maximum velocity of the piston in a Fielding engine, of equal power at the same speed of revolution, was not much more than 700 feet per minute. The pistons themselves were of slightly smaller diameter than the cylinder-bore, the rings alone touching the surface of the cylinder, the peculiar construction of the engine permitting this, from the fact that the pistons were firmly guided upon the pivots upon which they and the gimbal-ring were hung. This tended to reduce to a minimum the wear of cylinders, and compared favourably with what occurred in single-acting engines, in which the pistons not only touched, but actually had to serve as cross-head guides, and take the angular thrust of the connecting-rod. With regard to lubrication, he had found that a good "sight-feed" lubricator answered the requirements exceedingly well, preferably one having two deliveries of oil —one to lubricate the steam and the other to deliver to the passages leading to the main- and gimbal-bearings. These were shown on the sectional drawing (Figs. 5). By this means every part was thoroughly lubricated: the oil, passing after from the bearings, was thrown into the bottom of the casing, whence it might be drawn off and strained for further use. The construction of this type of engine compelled the adoption of forced lubrication, which was, he believed, the best possible system for high-speed engines, whether of the reciprocating or any other type. It also lent itself to the adoption of one central source of supply for the lubricant, which was a further advantage, the attendant having but one supply to replenish. On the matter of lubrication, therefore, he considered this type of engine stood at no disadvantage as compared with the reciprocating-engine, but rather the reverse, as it compelled the adoption of the most efficient system.

Mr. Brotherhood.

Mr. P. Brotherhood remarked that from an active experience in high-speed motor engines during the past thirteen years, he quite agreed with Mr. Imray, when he said that single action was to be preferred in high-speed motor engines, in order to avoid shocks resulting from alternation of strain at the working joints. In the year 1872 he commenced to work in this direction, and in 1873 exhibited at Vienna two engines, with three cylinders arranged at equal angles of 120° round a single crank. These engines were single-acting, live steam being admitted to the chamber containing the crank and connecting-rods, and pressing equally on the three pistons; the rods were subjected to a tensile strain, and their wearing-surfaces kept always in contact, rotation of the crank-shaft being produced by admitting and exhausting steam from the inner

Mr. Brotherhood.

chamber to the outer ends of the cylinders, by means of a rotating valve driven by the crank-pin direct. The speeds at which these engines could be run were far greater than any he had heard of doing practical work, and he made a considerable number on this principle for driving fans, pumps, and notably for centrifugal-machines, or hydro-extractors, some of which ran at 1,600 revolutions per minute, and did good work. Seeing his way to make improvements, he modified the original idea, and reversed the conditions, putting the steam on the outer side of the piston to make its stroke, and exhausting inwards, thus subjecting the connecting-rods to compression; this enabled him to simplify the details and get larger surfaces for wear. These engines had been improved in details as experience had guided him, and large numbers had been made. More than two thousand had been applied for propelling torpedoes by compressed air; three hundred and twenty for ordinary purposes; one hundred and four for driving steam-launches; four hundred and six for fans and centrifugal pumps; five hundred for working with hydraulic pressure; and seven hundred and twelve for driving dynamos direct, at speeds varying from 250 to 1,600 revolutions per minute. He exhibited one of the engines as used in torpedoes, and that little engine, weighing only 30 lbs., gave 20 actual HP. on the brake. He had effected several improvements in these engines since he first commenced their manufacture, and they were now made with balanced piston-valves, giving a somewhat increased efficiency, although at the sacrifice of some simplicity. During the past two years he had turned his attention to compounding these engines, and with very fair results. He exhibited a moving diagram of an engine constructed on the compound principle. It had a low-pressure cylinder of 6 inches diameter and 6 inches length of stroke, and the high-pressure cylinder was cast on the outer cover; the patterns and valve arrangements were such that he could make either simple- or compound-engines, as might be desired, from one set of patterns, compounding being done by adding the high-pressure cylinder, with its piston and rod, to the cover of the simple form of engine. The action of the engine was in this way. Suppose the pistons were at the front or inner end of the stroke, steam was exhausted from the low-pressure piston, and admitted to the under side of the high, the outer end being in constant communication with the steam-pipe; thus the exhaust stroke was made against the atmospheric pressure in the larger cylinder, and the high-pressure piston having steam on both sides was therefore in equilibrium, and opposed no resistance on the return stroke other than that which it might be desired to give, Mr. Brotherhood.

in order to cushion the moving parts. This was arranged by cutting off the steam in the inner end of the high-pressure cylinder at any desired point. The steam-stroke was made by the piston-valve opening from the inner side of the high-pressure to the low-pressure cylinder, and expansion took place, driving the crank with the additional force of the live steam at the back of the high-pressure piston. Thus the best results were obtained from the expansion of the steam between the two cylinders, and there was no excess of friction on the exhaust-stroke, due to having to force back the high-pressure piston against steam which was about to be expanded into a reservoir, or into a low-pressure cylinder. Several of these engines had been made, and were giving good results. He employed two at his works, running from six o'clock in the morning to half past nine o'clock at night, excluding the workmen's hour for dinner. These made 360 revolutions per minute, and had never required an adjustment since starting at the beginning of the year. Two others of the same size were driving the dynamos for lighting his shops, one running at 750 revolutions and the other at 650 revolutions, one giving 20 HP. and the other 25 HP.; they ran most satisfactorily, and with great economy, and had never given the slightest trouble. The construction of these engines was such that they were self-adjusting and took up their own wear; but this wear was guarded against as much as possible by using hard-steel surfaces in contact with phosphor-bronze, or white-metal; for instance, the cranks were made of Whitworth compressed steel, hardened so that no file would touch the surface, and finished between dead centres in a grinding lathe. Thus were secured true cylinders in the bearings, and this principle was followed in the construction of the whole of the engine, with the result that it was possible to take an engine as soon as completed, put steam into it, and run it at its full duty, within ten minutes of the first revolution, and this up to speeds exceeding 1,600 revolutions per minute, without the slightest sign of overheating or distress of parts. One of the latest improvements was a spherical piston. This consisted of a spherical shell which formed the end of the connecting-rod. The diameter of this shell was somewhat less than that of the cylinder, and the shell was encased in an outer cylindrical part, ground to fit the cylinder externally and to fit the sphere internally for a certain part of its surface. The principal part of the area of this piston exposed to steam-pressure was that of the sphere, sufficient area only being left in the annular envelope to ensure steam-tight contact. Thus the pressure was applied to the end of the connecting-

Mr. Brotherhood. rod direct, or through an almost frictionless joint, and thence to the crank-pin. The result of this arrangement was most gratifying. The outer shell being free to move circumferentially, gradually shifted round, and thus scoring of the surfaces was prevented, and they worked up to a perfect polish. He had taken advantage of this form of piston to make an additional exhaust-port, so as to do away with back-pressure as much as possible. An opening was provided on one side of the sphere, which, when the piston was making the steam-stroke, closed, and when exhaust was taking place, the rod being at the opposite angle, the aperture was open, and steam passed direct through the piston. By properly adjusting this opening the cushioning of the piston might be arranged as desired, independently of the steam-valve, and thus resistance and friction due to excessive back-pressure were avoided. This engine had advantages in addition to that of high speed. It had all the advantages sought in a rotary-engine, the shaft being in the centre, and it was so self-contained that it could be put to work in any position; for instance, bolted to a bulkhead, with its shaft horizontal, or above or below a deck in a ship, with its shaft vertical.

Mr. Willans. Mr. P. W. Willans said he had listened to both Papers with much pleasure, and as one of those most interested in the thorough discussion of the subject, he welcomed the last two conclusions which Mr. Imray had drawn. Those conclusions were an endorsement of the views which he had held and expressed, and they justified the course which he had pursued for the eleven years during which he had been connected with the design and manufacture of high-speed single-acting engines. So much had been said against these engines, mainly because the subject had not received the consideration which it deserved, that it was a great satisfaction to find even one man who gave it as his opinion that they were correct upon theoretical grounds, and not merely the best under the circumstances. He did not think, however, that Mr. Imray had adduced all that could be said in their favour in respect to economy. Mr. Imray had stated that if what he had urged about the effect of the weights of the moving parts was correct, there might be a question "as to whether there is any advantage in working engines in the compound way," and he also said that, "It may be open to discussion whether by the use of two equal cylinders of mean area, each provided with a very heavy piston, so that full advantage might be taken of inertia as an equalizing agent, results as good as those obtained from the compound-engine might not follow." Now, an equalization of strain, or perhaps, to put it better, a reduction of the maximum strain, was one of the con-

Mr. Willans.

sequences and also one of the advantages of working engines "in the compound way," but it was not the only benefit. The main advantage of compound-engines was not that the range of strains was reduced, although that was very important, but that the range of temperatures in each cylinder, and consequently the initial condensation, was reduced; and even if that which Mr. Imray said about the effect of the moving parts were wrong, which of course it could not be, engines would continue to be worked "in the compound way." He should like to point out that the equalization of the torque on the shaft throughout the revolution, though important, was not nearly so important in an electic-light engine on land as it was in a marine-engine used for driving a propeller, where a fly-wheel was not admissible. So long as the equal torque could be obtained without loss it was desirable to secure it by several cranks, or by arranging several cylinders around one crank; but experience with single-acting engines with one crank and one impulse per revolution led him to doubt whether that arrangement, coupled with a heavy fly-wheel, might not often be applied on the ground of simplicity and economy. Agreeing as he did in the main with Mr. Imray's conclusions, he of course disagreed with Mr. Kapp altogether. He believed that the economical results which would be obtained in the near future, by single-acting high-speed engines, would be better than any so far obtained with double-acting engines, at any speed at which it was practicable to run them for continuous work. He held that single-acting engines need not cost more, but should cost less for repairs and maintenance, and that there was not more but less need for a skilled engineer in their case than in that of a double-acting high-speed engine. The popular impression when he first put single-acting engines into boats (for, in spite of what Mr. Kapp alleged about marine-engineers, over three hundred of his engines had been put into boats before one engine was used to drive a dynamo), was that one cylinder was always acting against the other two, and that, therefore, it must be a wasteful engine. He did not know how many times he must have argued ineffectually that the power of the engine was increased when it was coupled to the condenser, although it would seem to be a pretty obvious fact that absence of back-pressure did affect the power. They were stated to be steam-eaters—a vague term which was particularly irritating. Nobody could understand them (mainly because nobody tried), and especially it was considered to be tempting Providence to drive any engine in a launch, unless the attendants could see the connecting-rods and get splashed with the oil from them, notwithstanding

Mr. Willans. that, in the matter of pistons and valves, people were happy to infer their presence from the result. These were a sample of some of the early prejudices against which Mr. Brotherhood, he, and others who had followed them had had to contend; and it was only now, when having come down in consumption by degrees from the early engines—which were practically without piston-rings and without expansion, and used a great deal of steam,—first to 50 or 60 lbs. of water per HP. per hour, then to 28 to 33, where they remained a long time, then to 22 or 23, and finally to about 20 lbs. in comparatively small non-condensing engines, that engineers had begun to think that there must be something in high speed after all. The means by which these results had been obtained had been, first, careful study of the forces to which Mr. Imray had directed attention; secondly, short passages and small cooling-surfaces combined with a good distribution of steam; thirdly, compounding; fourthly, a very efficient system of lubrication; and lastly, high speed.

He wished, first of all, to consider in what the advantages and difficulties of high speed consisted. To talk of piston-speed in this matter as Mr. Kapp had done was misleading: 600 feet per minute was a moderate piston-speed for an engine with 5 feet length of stroke, but to run 600 feet per minute with an engine having 6 inches length of stroke, and, as usually happened with electric-lighting engines, a very variable load, required some consideration. In any given engine, the difficulty encountered in checking the reciprocating-weights at the end of the stroke did not increase as the piston-speed, but as the square of the number of revolutions; and thus to run 600 feet piston-speed with 5 feet length of stroke, or 60 revolutions, entailed exactly one-tenth of the difficulties which were met with in the case of 600 revolutions and 6 inches length of stroke. This, of course, must be well known in principle, but he thought that Mr. Imray would have laid more stress on it. Even he spoke in a manner which might be taken to imply that piston-speed was in some sort a measure of the difficulty; and it was possible that several engineers present might have hardly realized its great bearing on the question. The speeds which Messrs. Willans and Robinson had adopted as ordinary working speeds for the present were:—

3-inch stroke . . .	870 revolutions.	435 feet piston-speed.
4-inch ,, . . .	760 ,,	510 feet ,,
5-inch ,, . . .	680 ,,	570 feet ,,
6-inch ,, . . .	620 ,,	620 feet ,,
9-inch ,, . . .	500 ,,	750 feet ,,
12-inch ,, . . .	440 ,,	880 feet ,,

Mr. Willans.

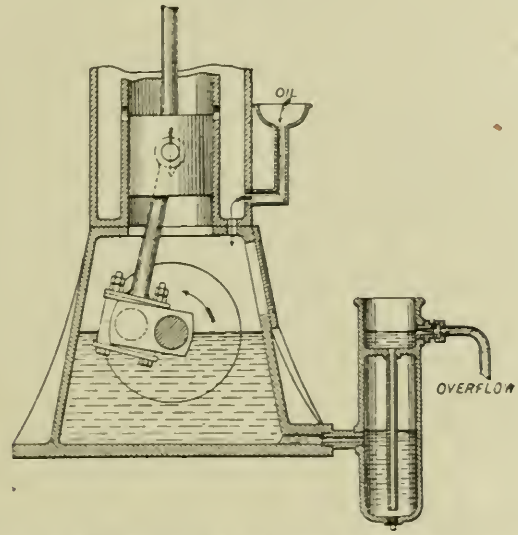
These speeds all presented about equal cushioning difficulties, and answered to speeds of 360 revolutions for 18 inches length of stroke, and 310 revolutions in the case of 2 feet. He hoped before long to exceed them; but these he knew to be satisfactory speeds for continuous running. The advantages of running at a high number of revolutions per minute were: 1st, a greater power could be got out of the same dynamo; 2nd.—he was not sure that this should not come first—high speed conduced to the economy of the engine. For instance, a greater power would have been obtained from Mr. Kapp's dynamo at the International Inventions Exhibition if it had been driven at the speed just named for a 5-inch stroke single-acting engine, viz., 680 revolutions. The total electromotive force, which was, as Mr. Kapp said, the great object in designing a dynamo, would have been doubled, and the commercial efficiency of the machine would have been higher, for the loss in the armature would have remained the same. Mr. Kapp said that the speed could be reduced without loss in commercial efficiency, but it appeared to him that the speed of any given machine could not be increased without a gain in efficiency. He thought that at doubled revolutions Mr. Kapp's machine would not have exceeded, or would hardly have exceeded, the peripheral speed which he himself named as the usual limit, viz., 3,000 feet per minute. This being so, he felt sure it would be admitted that there was an object, if it could be done, in increasing the speed from 340 revolutions to that just named, because the cost of the dynamo per lamp would be halved. There was also great economical advantage in increasing the revolutions of the engine. In highly expansive engines, one of the principal losses was the initial condensation, and the best way of reducing that loss was to accomplish the greatest possible amount of work in a cylinder with the least possible surface, the smallest variation in temperature, and in the shortest possible time. He had enlarged two sets of diagrams from one of his latest compound-engines, indicating about 40 HP. for each 100 revolutions. One of these sets of diagrams was taken at 200 revolutions, and the other at about twice that speed, and the action of the initial condensation in raising the expansion-curve in the low-pressure cylinder of the low-speed diagram was very marked. The diagrams showed only 49° range of temperature in the case of the high-pressure cylinders, so that the effect of condensation was slight compared with what it would be in a non-compound-engine with the same total range of expansion, but it would illustrate his meaning. Thus the economical and commercial advantages of high rotative speed should be very great, apart from their other

Mr Willans. advantages such as small size and steady running. Mr. Charles T. Porter, of Philadelphia, had said, with reference to the advantages of high speed:—"These are practically confessed by almost all builders, who now aim generally to run their engines as fast as they consider to be prudent, each one, however, disapproving very strongly of speeds above that to which he finds himself limited," and Mr. Willans thought the same remarks would apply to engine-builders here.

The next point to which he would refer was his system of lubrication. Here again there seemed to be a misunderstanding, for single-acting engines and closed-engines appeared to be considered by some people to be convertible terms. Now, the reason Messrs. Willans and Robinson had closed their engines, as Mr. Kapp called it (although they had not, as he said, put them in a tight casing, for there was always an inch hole in it through which the attendant could pour oil, without the certainty of a part of it being returned to him promptly by centrifugal action, as in the case of unprotected engines), the reason why they covered their engines was to protect them, and render the lubrication less difficult, and it was the absence of the necessity for minute adjustment in a single-acting engine which allowed (and not compelled) them to do this. They knew by experience that with suitable oils there was a diminution rather than an increase in friction up to nearly the boiling-point of water. The boiling-point of water was also the best possible safety-valve in this case, because, although it was only rarely reached, it could not well be exceeded. They therefore ran the bearings in oil and water mixed, and the result, in saving of oil, absence of friction, and reduction of attendance, was excellent. The flooding of the bearings with oil was a very great point. They had arranged a form of gauge by which the height of lubrication in the crank-chamber could be roughly ascertained, and by which also the water could be separated from the oil, and any surplus water could be run off or fresh water added (Fig. 6); for it must be understood that they contemplated running these engines, not for a few hours at a time, as at the Exhibition, but month after month continuously. This lubricating arrangement was shown in Fig. 12. The engine was thus protected only because there was no reason for its being otherwise, and he had no objection to making unprotected engines except that they were not so good; he had often been asked to do so, but in no single case had the desire for an open engine survived the explanation of his reasons for closing it in. The system was perfectly trustworthy, and as no bearings needed to be adjusted from year's end to year's end, the crank-chamber door had only to be opened occasionally. He asked the members to consider the complicated arrangements for centrifugal and other lubrication in open double-acting high-speed engines, and to compare with these the certainty and simplicity of the lubricating arrangements in the protected engine, Fig. 12. Mr. Willans

Before turning to the principal part of Mr. Imray's Paper, he should like to refer to compounding as applied to high-speed engines. Mr. Imray said: "Of late, Mr. Brotherhood and other makers have been constructing three-cylinder engines with duplicate cylinders to act as compound-engines." Mr. Willans believed that the first compound-engine on his system was one of the first compound-non-condensing engines made. It was built by Messrs. Hunter and English, of Bow, in 1875, and promised so well from the economical point of view, and entailed so little additional cost, that they made many more, and from that date he had never willingly built any other kind where the pressure available permitted it. The first of his engines which were compounded were made with the high- and the low-pressure pistons actuating different cranks; in one type there were two low-pressure cylinders, and one high-pressure, on three cranks, and in another type one high-pressure and one low-pressure cylinder on opposite cranks with one piston-valve, as in the Westinghouse engine, except that it was a compound-engine. He soon found, however, that to make a good compound-engine the

Fig. 6.

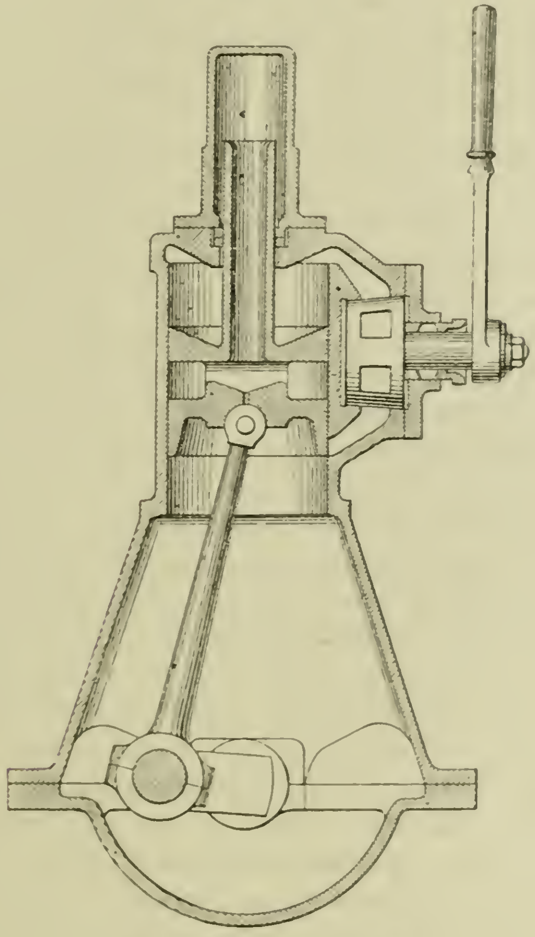


Gauge for Height of Water in Crank-Chamber.
Scale $\frac{1}{16}$.

Mr. Willans. high-pressure and low-pressure cylinders must be placed tandem, on account of the very small share of work done by the low-pressure cylinders of non-condensing engines with light loads, when the steam was expanded nearly to, or even lower than, the pressure of the atmosphere; and in all compound-engines which might at times have to exhaust into the atmosphere, it was inexpedient to lose sight of this fact. These tandem compound-engines were a compromise between economy and other mechanical advantages, and as the method of cushioning the working parts was a peculiar one, and the forces with which Mr. Imray's Paper dealt were carefully considered in its design, with the result of leading up to an entirely different method of cushioning which they had recently adopted, he had prepared a diagram explanatory of its action. From this it would be seen that in each high-pressure cylinder a certain weight of steam was compressed and allowed to expand during each revolution, and that the compression commenced at about one-fourth of the upstroke. The advantages of this system of compression were that it was economically applied, for the loss was trifling; it followed fairly well the "inertia-diagram," and for the purpose for which the engine was designed, viz., marine work up to 120 or 150 indicated HP., where the pressure required to turn the screw increased with the speed, it answered its purpose in every respect, for the cushioning also increased with the pressure. It was always, of course, obvious that whatever the system of cushioning, it must commence at or before the middle of the upstroke, and he had in the early days of the engines used the pressure on a tube as in Fig. 7; and subsequently, when the valve and piston were separated, the pressure on the piston-valve, to attain the desired end. This arrangement of a continuous downward pressure during the whole stroke entailed more friction, and had no tendency to equalize the strains. It was in designing the tandem compound-engines that he first saw the great advantage in a suitable cushion in equalizing the strains, which were rendered unequal by the starting and stopping of the moving parts, and it would be interesting to Mr. Imray to know that for a long time he had constructed inertia-diagrams for each new engine, in order to decide the necessary cushioning to keep the connecting-rods on their bearings without greater pressure than was necessary. It would be well to consider what the difference was between the two types of engine, double- and single-acting. In double-acting engines the strains were transmitted to the crank by the alternate push and pull of the connecting-rod. In order to oil the bearings it was essential that there should be some clearance, however small, between the

Mr. Willans. crank-pin and its bearing, and between the small end-pin and its bearing. At one part of the stroke it was necessary that the direction of pressure should change, and owing to the slack the change must be made gradually to avoid noise and shock. There were two ways of doing this: the first was by compression or the gradual accumulation of pressure in front of the piston, so that by the time it reached the end of its stroke, the connecting-rod which pulled at the crank-pin hitherto had commenced to push it in the reverse direction. The second method was advocated by Mr. Porter, who proposed to make the reciprocating-parts of an engine so heavy that the force which they exerted on the centre was greater than the pressure of the initial steam. In this method, the cushioning was of secondary importance. The weight of the reciprocating-parts preventing their being put in motion suddenly,

FIG. 7.



FIRST WILLANS ENGINE.

Scale $\frac{1}{6}$.

Mr. Willans. the connecting-rod at some point after the crank had turned the centre, overtook the crank-pin gently and without shock. These two methods were both in certain cases right, but each had its weak points. They were diametrically opposed to one another, for in the first method the working parts could hardly be made too light. In the second, the difficulty was to make them heavy enough. It was to the latter method that Mr. Imray's remarks principally applied, and as he brought forward this theory to some extent as a new one, he should like to point out that, although it might be new to some, it had nevertheless received a great deal of attention. He believed its first advocate was Mr. Charles T. Porter, in America, and the first record of it which, so far as he knew, was published in this country, was a reprint in "Engineering," of a Paper by Mr. Porter, read before the Polytechnic Club of the American Institute.[1] Mr. Porter's object was to increase the weight of the moving parts, but he probably had no idea in 1871 of the pressure which would be employed generally in the year 1885. In a recent work, in which he explained the construction of the now celebrated Porter-Allen engine, Mr. Porter said:—

"For example, in an engine of 2 feet length of crank, making 50 revolutions per minute, it is 1·705 times the weight of the reciprocating-parts; for $50^2 \times 2 \times 0{\cdot}000341 = 1{\cdot}705$. That is, they are being put in motion 1·705 times as rapidly as gravity would do. We have then only to multiply the weight of these parts by 1·705, and we have the total force; and then by dividing this by the number of square inches on the piston, we have the pressure of steam per square inch that will furnish it. At the above speed it will be insignificant, probably 7 or 8 lbs. per square inch. But it increases as the square of the speed; at 100 revolutions it is four times as much; at 200 revolutions it is sixteen times as much. The first is 30 lbs., something to be considered; the second is 120 lbs., something to be avoided."[2] Thus, then, the originator of the theory, or, at least, the man who first applied it to double-acting engines, contended that it was not applicable to pressures of 120 lbs. per square inch. Engineers now had to deal every day with such pressures as 120 lbs., and probably would soon have to deal with double those pressures. As an illustration, it would be well to consider what weight the moving parts of the engine shown in Plate 2, Figs. 27 and 28,

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[1] "Engineering," vol. xi., 1871, p. 202.

[2] "Description of the Porter-Allen Steam-Engine." By Charles T. Porter, 1883, p. 30.

Mr. Willans.

would have, to overcome at 700 revolutions the very usual pressure of 120 lbs. per square inch. This speed, with a length of stroke of 5 inches required, at the end of the stroke to overcome the steam-pressure, a force equal to thirty times the weight of the moving parts. The initial pressure amounted to 4,000 lbs., and the weight of the working parts would have to be over 130 lbs. in order to overcome that pressure. The piston might therefore be composed of a solid block of cast-iron, double the capacity of the working cylinder, and still leave plenty of weight over for the connecting-rod, piston-rod, and cross-head. Engineers must, therefore, be careful how far they applied Mr. Porter's theory, for the balancing of such weight would be necessary, and would probably entail considerable difficulties. With respect to the balancing of reciprocating-engines, Mr. Tower had illustrated his remarks by a most unusual case, namely, that of an engine with a length of stroke of 6 inches, turning 1,000 revolutions per minute, and having connecting-rods 9 inches long. He had experienced no trouble with his own engines in this respect up to the speeds which he now adopted, and he had not balanced them, or at any rate only so far as they were naturally balanced by placing two cylinders side by side, the pistons in which were attached to opposite cranks. It was, of course, a matter which at very high speeds would give trouble; but he did not anticipate any practical difficulty in running an engine with 6 inches length of stroke at 800 revolutions per minute, with connecting-rods of a reasonable length. At such speeds the reciprocating-weights would require to be partially balanced.

The other method of reversing the strains in an engine gradually was known as compression, and was the method generally adopted by engineers in this country, the aim being usually to make the working parts as light as was consistent with strength. The reason of this was that compression, although an aid to economy if properly applied, might if excessively and improperly applied occasion a loss, and in high-speed engines it might be a large loss, because the power necessary to put in motion and to stop the working parts was a large part of the total energy exerted by the engine. For instance, in an engine where the weight of the working parts must be multiplied by 30 to give the centrifugal force on the centre, the inertia-diagram would be 25 per cent. of the indicator-diagram, when the mean pressure of the latter was 30 lbs. per square inch. If compression was always carried up to the initial pressure, as it might in an engine working with some system of variable expansion which did not affect the exhaust, Mr. Willans. there was a liability to difficulties when the steam-pressure varied, or when, as often happened in a ship, the engine was required to exhaust into the condenser, and there was consequently little or nothing to compress. It was also worth noting that in any engines variable expansion could only be carried with economy to a certain limit; and in his opinion, with the non-condensing compound-engines likely to be generally used for electric-lighting, the cut-off would, if anything, have to be later, not earlier, as the load decreased, if economy was studied at the full load. Before leaving this part of the subject he should like to point out that an automatic valve-gear which, like the Westinghouse, increased the compression with light loads, was economically right, *i.e.*, if the compression could be arranged to increase in proportion to the expansion; but from the present point of view it seemed to him to be wrong; because the back-pressure of the atmosphere or condenser was constant, and if the weights and speeds were so proportioned that with the minimum compression and at the maximum load the engine ran noiselessly, there must be too much compression with the lighter loads. Or, if the compression was correctly calculated for the light loads, there must be insufficient compression and a knock (if the bearings were slack) with heavy ones. Throttled engines were even in worse case. A locomotive and an electric-light engine were not parallel instances. In a locomotive the maximum mean pressure, and consequently power per revolution, was rarely required at the maximum speed. In an electric-lighting engine the speed was constant, or for the purposes of this inquiry might be taken as constant, under both light and heavy loads. What was required, if it could be attained, was some system of compression constant at all loads, because the weights of the moving parts could not be altered when the lights were switched out. A compression-diagram was wanted somewhat larger than the inertia-diagram, and in electric-lighting engines it was very necessary that this compression-diagram should not be affected by changes in load and initial pressure, nor by varying conditions as to the destination of the exhaust steam. It would readily be understood that where so much depended upon compression, such conditions as those imposed by the Admiralty on all engines for electric-lighting were very difficult to fulfil, these conditions being that the engine should work equally well when exhausting either into the condenser or into the atmosphere, and with full load and no load. These considerations induced Mr. Robinson and himself to adopt the only method of cushioning, which was a constant one, viz., a cushion apart

Mr. Willans.

from the working cylinders, and it was thus open to them to adopt the most economical compression in the steam-cylinders, independently of all question of cushioning properly so called. In single-acting engines there was this great practical advantage, that the exact amount of compression did not matter so long as there was sufficient. He meant, that if the connecting-rods never left the bearings, they could not come back again with a thump. Still, it was none the less desirable in their case that the compression should not be excessive, and should remain constant, rather than vary with the pressure or back-pressure. The method adopted so far, although many other methods naturally suggested themselves, was to use the circular guide or cross-head (which they were then introducing in order to avoid the necessity of jointing the connecting-rod into the working-piston) as a sort of air-buffer. This was not a new working part, for they had long been satisfied that some separation between the steam-cylinders and crank-chamber was necessary, especially in large engines. This guide had so far been used without rings, being deep and efficiently lubricated by the oil in the crank-chamber. Near the bottom of each stroke it passed some openings in the side of its cylinder, which communicated with the outer air. These passages being closed at the commencement of the upstroke, the air was compressed as the piston rose, only to expand again on the down-stroke without sensible loss. The complete separation of the crank-chamber and working-cylinder, which the presence of this separate cross-head, or guide, enabled them to make, permitted the employment of suitable oil for the crank-bearings without the necessity for using it also in the boilers. Engineers were very careful, and rightly careful, as to the oil which was used in the cylinders of any engines exhausting into a surface-condenser, and should be equally so in the case of a crank-chamber with which cylinders were in communication. This complete separation enabled them to use the minimum of oil in the cylinders, while they were in no way tied as to the quality or quantity of the oil for the bearings.

With reference to the question of inertia having been already investigated, in "The American Machinist" of the 10th of May, 1884,[1] and in other numbers of that periodical in the same year, there was a letter from Mr. E. F. Williams, explaining how he took, by means of a specially designed instrument, a diagram of

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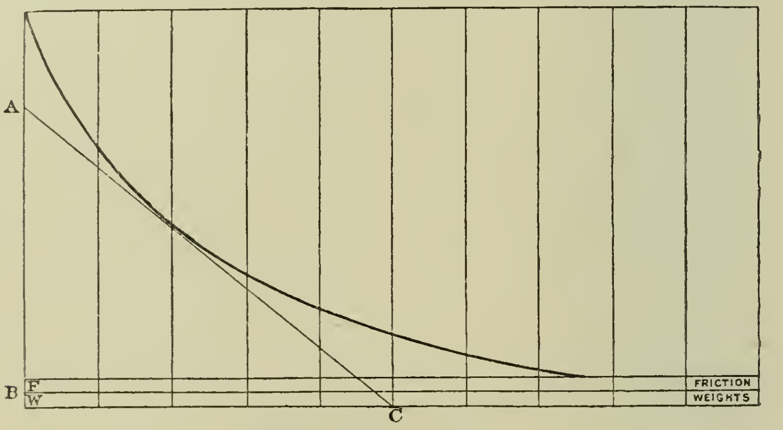
[1] Vol. vii., No. 19, p. 5.

Mr. Willans. the inertia of the reciprocating-weights of a steam-engine, and the diagram was reproduced in that periodical.

To return to the inertia-diagrams. He did not usually take the connecting-rod's angular effect into consideration. His connecting-rods were longer than those which Mr. Imray took, and which appeared to be very short; he used connecting-rods from four-and-a-half to five times the length of the crank; but he accepted the formula for centrifugal force as correct, which it would be if the connecting-rods were of infinite length. Treating the whole mass of pistons and connecting-rods as if it revolved with the crank-pin, he set off the resultant centrifugal force in lbs. pressure on the cylinder area, as the line A B (Fig. 8), and joined A C. The triangle A B C gave roughly the area of the inertia-diagram.

Fig. 8.



Air-Buffer Diagram.

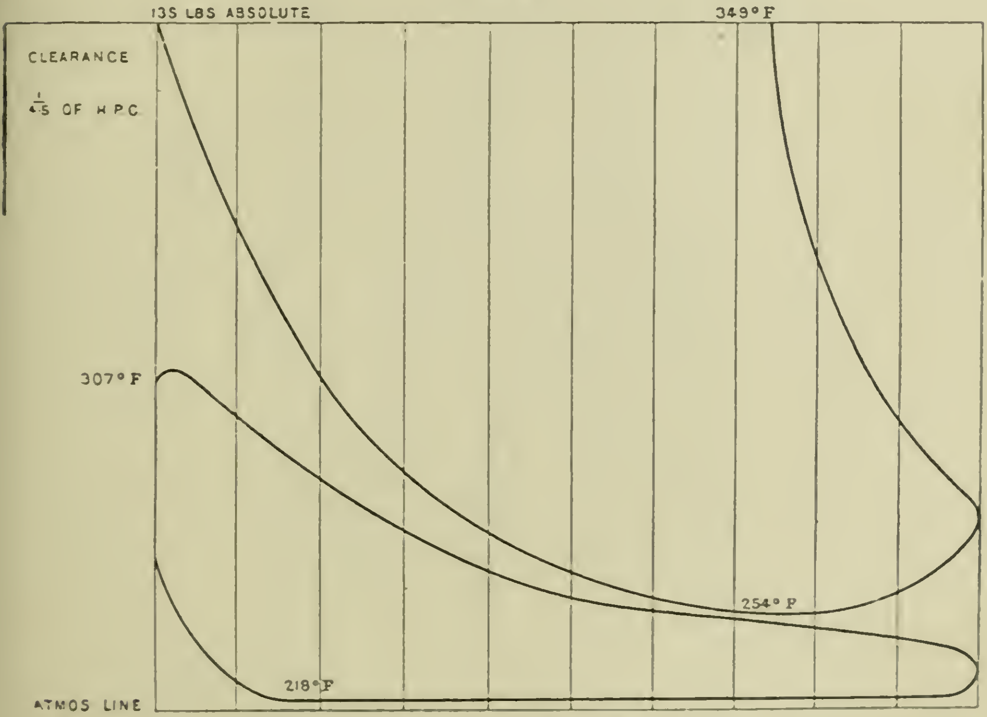
Centrifugal force in lbs. pressure per square inch on L. P. cylinder area.

Although Mr. Imray explained the effect of inertia, he did not show how to apply the theory in the case of a single-acting engine. Fig. 9 exhibited one way in which Messrs. Willans and Robinson managed this, before they used the later method of cushioning of which he had spoken; but this was limited to cases where the pressure and the speed increased together, and where there was no chance of the engine having to run at any very high speed with a very low pressure. This was important, for no compression varying with initial pressure fulfilled all the conditions required in an electric-lighting engine; it was either too much at one time, or too little at another, if the load varied much. The method which he adopted in determining the proportions of the air-cushion in a vertical engine, was to lay off a line W parallel to the base-line of the inertia-diagram (Fig. 8),

Mr. Willans.

representing the weights of the moving parts in lbs. per square inch on the cylinder-area; above this another parallel line F representing friction; and above this the cushion, or air-buffer-diagram, just touching the inertia-diagram. Fig. 10 represented the effect of the double air-buffer and the double mass of weights which were started and stopped at each forward stroke in the case of a double-crank engine. He had not taken the angle of the connecting-rod into account, but treated it as if it were of infinite length; and this was practically right in the double one, as the two triangles were different. The figure A B C D showed the combined pressures of the high- and low-pressure pistons during the down-stroke. The air-buffer-diagram E acted with the steam, and the double inertia-diagram F F¹ helped the reduced steam-pressure at the end of the stroke. The double inertia-diagram G G¹, which had to be deducted from the total area, and the air-buffer, diagram H, which checked the weights on the up-stroke, were sectioned, so that the unsectioned area of the diagram represented the net effective pressures during the stroke. At a higher speed the diagram of net pressure became still more parallel. In the old compound-engine, Fig. 11, some economy was sacrificed to

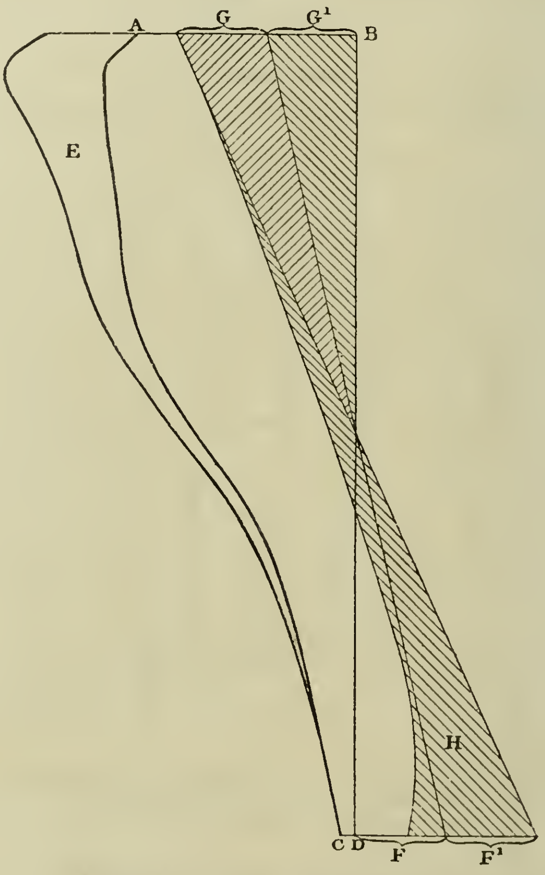
Fig. 9.



Indicator Diagram from Fig. 11.

Mr. Willans. mechanical considerations. One of the advantages of the arrangement was that it permitted of the necessary cushioning without waste, but no compound-engine really took advantage of all the possibilities of compounding which did not contain a receiver, and thus place a complete barrier between the temperatures in the two cylinders. In his old engine the range of temperatures in the high-pressure cylinder, as in all engines without a receiver, was measured by the difference between the temperature of the initial steam in the high-pressure cylinder, and that due to the pressure at the point of cut-off in the low-pressure cylinder, and consequently was much greater than in a receiver-engine.

Fig. 10.



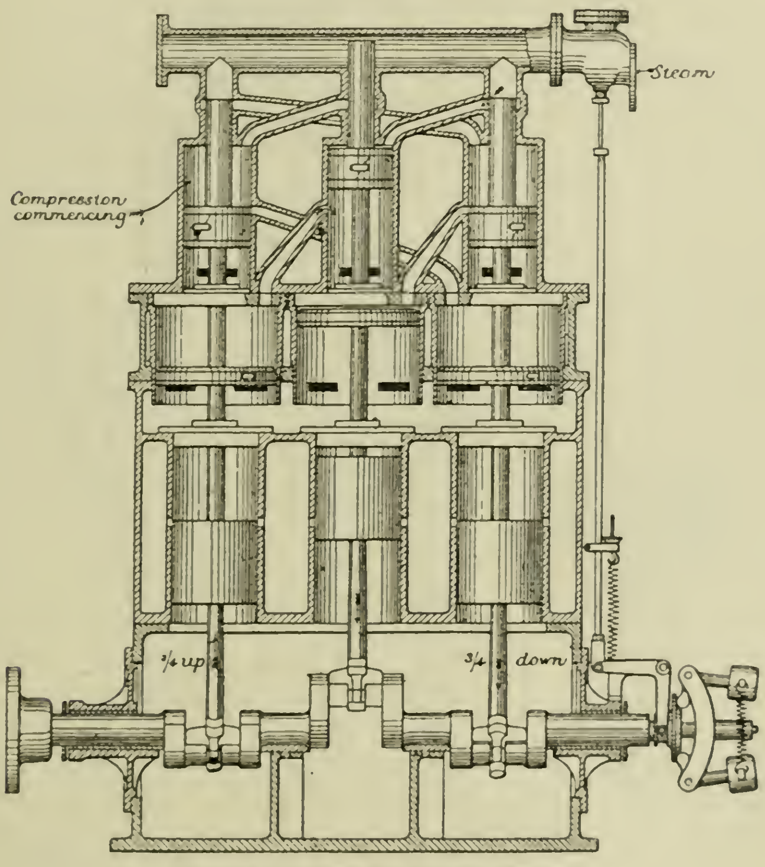
Inertia Diagram from Fig. 12.
400 revolutions.
All diagrams reduced to area of L. P. cylinder.

Fig. 12 showed his latest form of compound-engine. In this he had been untrammelled by all questions of compression, and it had been open to him to minimize clearance and passage-surface as far as possible, and also to introduce a perfect form of valve-gear. It

Mr. Willans.

was only necessary for the purpose of understanding the diagrams, Figs. 13 and 14, to say that the valve in the engine was a piston-valve in the piston-rod, upon which the high-pressure and low-pressure pistons were mounted; it was actuated by an eccentric, not on the crank-shaft but on the crank-pin, for the valve-face being movable the centre upon which the eccentric turned must follow it about. The cut-off was obtained by the passage of the port (through which the steam entered the hollow rod) into the metallic rings in the cylinder ends. The cut-off was thus effected at the full speed of the piston. It could readily be varied, and Messrs. Willans and Robinson were now making a large number with expansion-gear variable by hand for ship-lighting, in order that when working with low pressures the ratio of expansion might be reduced. The model, which he exhibited, would explain the action of the valve-gear. The upper side of the high-pressure piston was the working cylinder; the steam was admitted by the valve

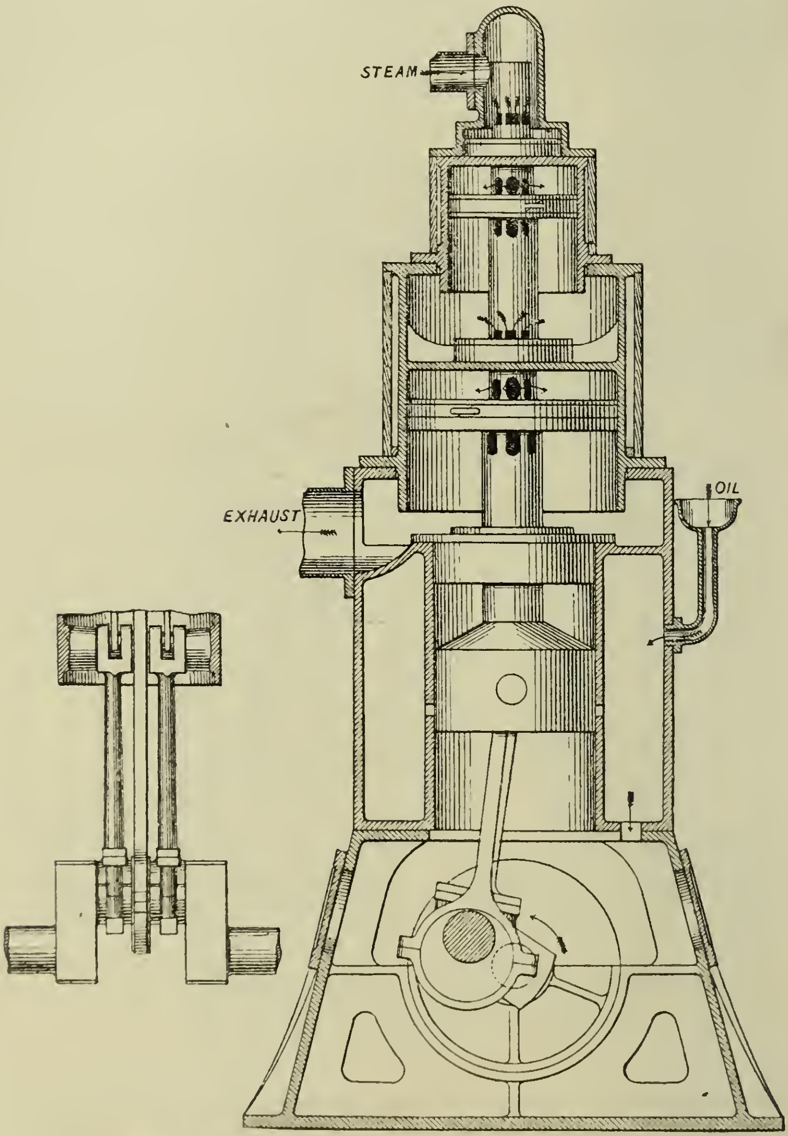
FIG. 11.



TRIPLE-TANDEM COMPOUND-ENGINE.
Scale $\frac{1}{12}$.

Mr. Willans. in the piston-rod, was cut off by the direct motion of that rod, and, being released again at the end of the stroke by the valve, it passed into the space below the high-pressure piston, which formed the receiver; thence in the same way it passed from

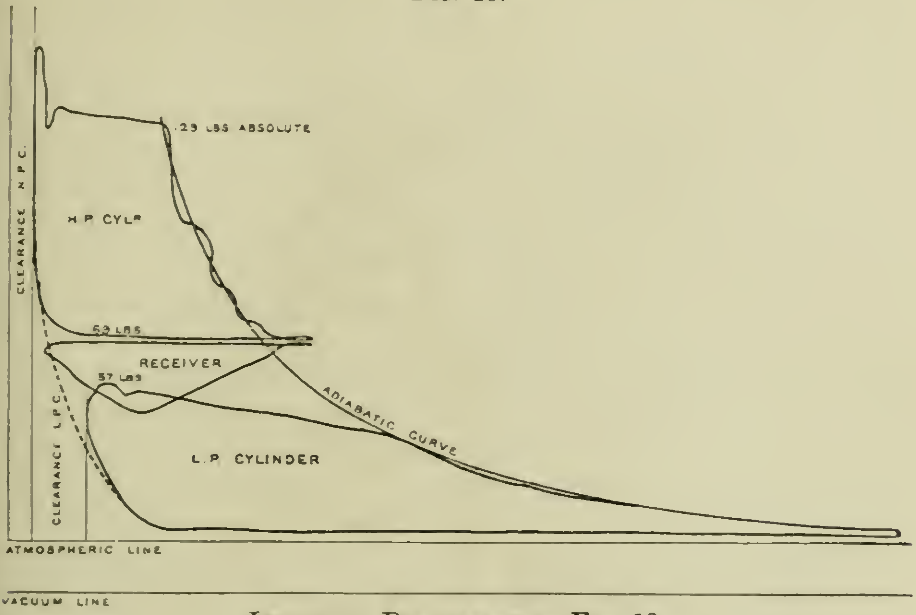
Fig. 12.



Double-Crank Compound Receiver Engine.
160 . I.HP. at 400 revolutions, 200 I.HP. at 500 revolutions.
Scale $\frac{1}{20}$.

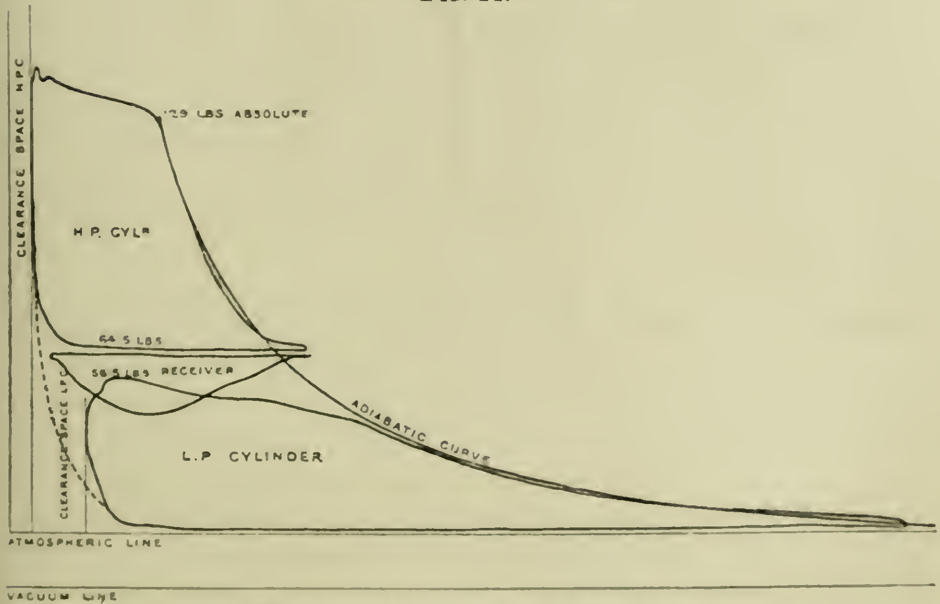
the receiver into the low-pressure cylinder, in which the ratio of expansion was nearly proportional to the ratio of the areas of the two cylinders. From there it was allowed to escape into the exhaust. The cylinders from which the diagrams had been taken were unjacketed; the range of temperatures in the high-pressure cylinder was about 49°, that in the low-pressure cylinder 50°. Mr. Willans.

FIG. 13.



INDICATOR DIAGRAM FROM FIG. 12.
High-Speed.

FIG. 14.



INDICATOR DIAGRAM FROM FIG. 12.
Low-Speed.

The variations of pressure in the receiver had a considerable effect on the power of the engine, so that a diagram had to be taken from

Mr. Willans. it as well as from the high-pressure and the low-pressure cylinders. The steam entering at the top, the water formed in the cylinder passed steadily down through the engine, and no water could collect while it was at work, except in the receiver, which was arranged to be drained.

There was another arrangement in which the compression was somewhat similar to that in his older engines, and in which the under side of the high-pressure piston was used as the working cylinder, the boiler steam being constantly above it. The steam being cut off early in the high-pressure cylinder and expanding, an effect similar to compression was obtained, because the steam above the high-pressure piston remained constant, while that beneath it was reduced in pressure as the piston moved upwards. He believed that in certain special cases this method might possess advantages; for instance, where the pressure in the steam-chest had to increase with the speed, as in a boat, but it could not be so economical as the compound-receiver-engine. By a coincidence, Mr. Brotherhood and he both described such an engine in their specifications on the same day, but with different arrangements of valves. Mr. Brotherhood had described the engine, but he had not given any figures as to economy. These would have been very interesting in comparing the two types of engine. He had tried to show that no arrangement of valve-gear could meet the case of varying pressures, varying loads, and varying back-pressure. But it could readily be shown that even in cases where all the conditions were determined beforehand, no double-acting engine could run the same speed as a single-acting one, with equal freedom from vibration and noise, if high-pressure steam were used, especially when it was considered what a slight amount of wear affected compression.

He wished to make one point clear about which there often seemed to be misapprehension. It was that single-acting engines were not capable of the same speeds as double-acting engines for a short run. For such time as the bearings of a double-acting engine would hold out, there was hardly any limit to its speed; but single-acting engines, the bearings being slack to begin with, were limited to the speed at which the connecting-rods ceased to press on them, and it was therefore the more necessary to consider their design carefully.

The following were his reasons for preferring single- to double-acting engines for electric-lighting and similar purposes:—

1st. The reciprocating-weights of the latter could not be cushioned with economy or certainty when the steam-pressure was variable, or very high, or when the back-pressure was variable or very low; Mr. Willans. and in consequence of these difficulties it was impossible to run them at such speeds as would give the most economical results in first cost of the dynamo.

2nd. The single-acting engines were more economical; and Mr. Kapp's objections to them on this ground would, he thought, have had more weight if he had given a little information as to the economy of the engine, Plate 2, Figs. 27 and 28, upon which he pinned his faith.

3rd. The speed of the engine was more regular, a two-cylinder single-acting engine, running 680 revolutions per minute, having the same number of impulses per revolution, but twice as many per minute, as the Allen engine running 340 revolutions.

4th. The lubrication, attention, and maintenance required for single-acting engines should entail far less expense than the same items in the case of double-acting engines; and

5th. In the matter of governing, high-speed engines had also an advantage, as the measure of variation in speed in a throttled engine was the clearance between the throttle-valve and the cut-off-valve face. Assuming, in the case of the high-speed engine, the same clearance in proportion to the capacity of cylinders as in a low-speed engine, the action of the governor must be more immediately felt.

With respect to radial cylinders acting on one crank, as compared with parallel cylinders acting on two or more cranks, he had always thought that a standard engine of the single-acting type should have parallel cylinders, because in that arrangement alone the strains were always, as it seemed to him that they should be, in one direction. He did not want to say that Mr. Brotherhood, and those who had imitated him, were wrong in all cases. There were several cases, as for instance torpedoes, where other than radial engines were hardly possible, but he meant as a general rule. He believed that in the radial arrangement some slackness in the coupling would be essential, and while admiring the ingenuity with which Mr. Brotherhood had surmounted the difficulty, he thought that prevention was better than cure. The one point upon which an argument against single-acting engines could with some show of reason be based, was that the descending piston must not only overcome its own friction, but that of the ascending one. This was a purely practical question, and he could only answer it by saying, that the performance of single-acting engines, so far as he knew, had at least equalled all previous records in the matter of absence of friction, and these engines could be counted on for at

Mr. Willans. least 90 per cent. of the indicated-power with 40 lbs. mean pressure. He had taken diagrams from his engines with 12-inch cylinders in a yacht which showed only 1½ lb. mean pressure to turn the screw; and light diagrams taken from an engine similar to Fig. 12 when reduced to the low-pressure cylinders, showed a mean pressure of 3 lbs. per square inch. He might add that the piston-rings in this engine were not the apologies of rings too often considered good enough for high-speed engines, but that they fitted the cylinders tightly, and when the light diagrams were taken, the engine had not had a week's running. The last of his engines which had been carefully tried, in connection with one of Mr. Crompton's dynamos, gave an electrical output, or commercial efficiency, equal to about 80 per cent. of the power shown on the indicator-diagram (Fig. 13), a result probably equal to one electrical HP., for or under 3 lbs. of coal. Where these engines had been tested on the brake against double-acting engines, they had always beaten them; but whether on account of the shock in the latter when the strains were reversed, or whether in consequence of the advantage of the system of lubrication employed in the former, he did not know. Mr. Kapp had stated that in consequence of the increased diameter of his armature, and of the more powerful field-magnets now used, nothing stood in the way of the employment of a "good substantial double-acting engine." There was something insinuating about this succession of adjectives, but he felt sure that, if the type of engine which he advocated could be made more economical and easier to manage, it would be at least as "good," and if, at first sight, any part appeared less "substantial," he hoped Mr. Kapp would remember that the strains which it had to bear were considerably smaller.

Mr. Jones. Mr. E. Jones believed the only point of novelty about the invention illustrated in the diagram of the Armington-Sims engine, was that of regulation by means of a cut-off. The disk carried two levers which formed balance-weights. There were two eccentrics, the inner one free to turn on the shaft, the outer free to turn on the other, and they were connected to the balance-weights, which always fixed their position. When the levers were quite down, the eccentrics were at their longest stroke; one was turned in one direction by the levers, and the other in the opposite direction. When the levers were thrown outwards to the largest extent, the valve had a shorter stroke, and only just allowed steam to be admitted, cutting off again immediately. The action of the levers altered the position of the two eccentrics with relation to each other, and increased or shortened the stroke of the valve according to the amount of steam required Mr. Jones. to go into the cylinder. The difference between the steam being admitted up to about half-stroke, and being cut off immediately at the point of admission, was produced by the short distance that the levers had to move. They were acted on immediately by the slightest alteration of the speed of the engine, and the regulation of the cut-off was produced accordingly. The engine was specially constructed for running at a comparatively high speed, and provided with very large bearing-surfaces. The piston-speed

Fig. 15.

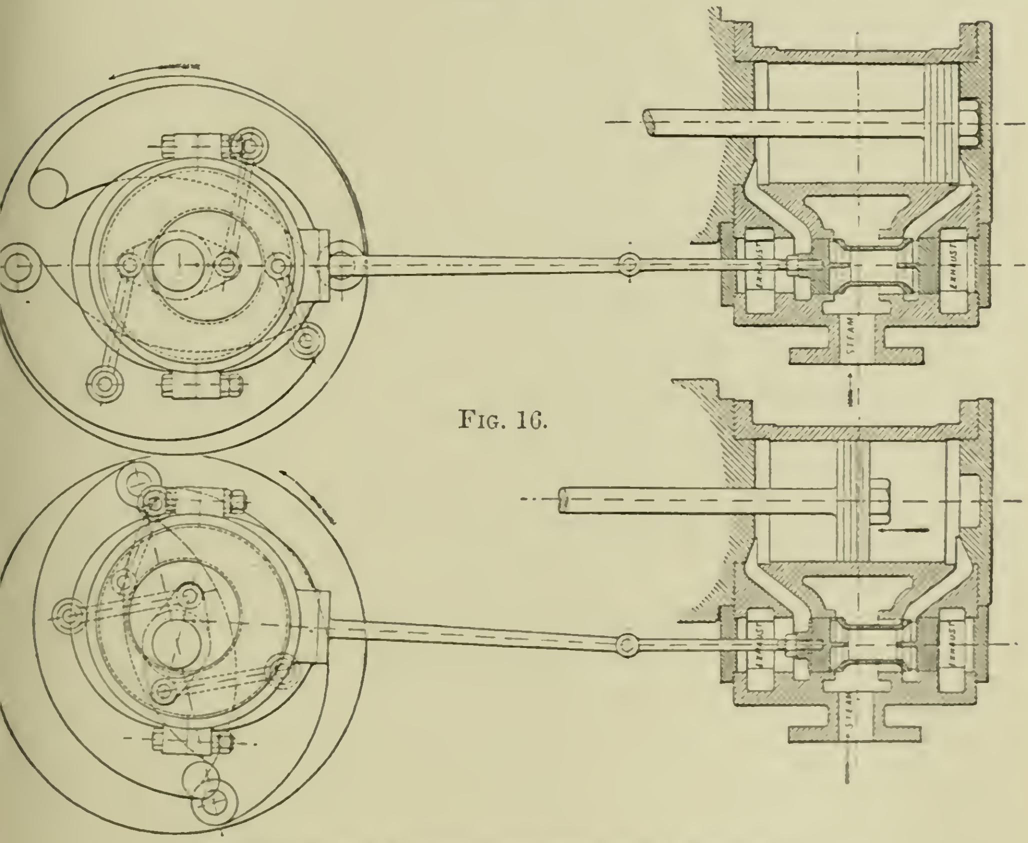


Fig. 16.

Regulator for Armington-Sims Steam-Engines.

Scale $\frac{1}{10}$.

in many of these engines exceeded 600 feet per minute, and the number of revolutions was 400 per minute in some sizes. There were now more than one thousand of these engines driving dynamos in America. Fig. 15 showed the cut-off as soon as the steam had been admitted, the levers being at their greatest outward extent. Fig. 16 showed the cut-off acting when the piston had travelled about half its stroke, the levers being at their shortest distance from the centre.

Mr. Allen. Mr. W. H. Allen observed that the members had had an excellent opportunity of seeing in practice the work of rotary- and reciprocating-engines driving dynamos and kindred machines at the recent Inventions Exhibition at South Kensington, and he presumed that they had all come to the same conclusion as Mr. Imray had arrived at, namely, that the rotary-engine at the present moment was not fit for general adoption. It had the same inherent defects as those manufactured years ago, the uncertainty of rotation, the difficulty of packing, and the consequent waste of steam. Any visitor to the north-east corner of the Exhibition, where the rotary-engines were congregated, was treated to what might be considered a roll of musketry, in examining the exhaust-pipes of the engines at work. The steam shot out into the atmosphere to a height of 30 yards. For purposes of comparison, there were several compound-engines close by, from which the vapour passed off quietly, all energy having expired. The rotary-engine must attain far greater excellence before it could be used for general purposes. The three-cylinder engine had been in the market fifteen years, and nothing could be said against it. It had been developed from time to time under the hand of the inventors, Mr. Peter Brotherhood and Mr. Willans, and in their hands it worked exceedingly well. There was, however, a serious defect in these engines, which would always close them against some engineers; he alluded to their inaccessibility. A large number of the community was always purchasing engines, a certain section of which would not hesitate to purchase engines of that description; but there was another and a larger section who generally weighed matters before embarking on any commercial transaction, and they said to themselves, "If we purchase an engine, we want one that we can readily comprehend, and that could be handed over to an ordinary mechanic to repair." If the engines to which he had referred were not fit for adoption, there was nothing left but the ordinary reciprocating-engine, and the question to be considered was, Why could that not be made to work at fairly high speeds without difficulty? He had made a large number of engines of that description with single cylinders, and with double cylinders, and he had never known one engine that had not fulfilled its duty from first to last. Mr. Tower had given a theoretical sketch of the methods of balancing reciprocating-engines, but which was not borne out in practice, and an excellent opportunity had been afforded for testing the direct-acting engines and dynamos lately built by Messrs. Allen and Co. for the "Devastation." The engines were simply laid on the floor; the cylinders were 16 inches in diameter, and at 300 revolutions per minute they worked with an entire absence of vibration. He was satisfied that with an engine made in that way there was no practical difficulty. In order to test whether the balance was perfect, the caps were loosened from the front-bearing, and the middle-bearing between the two engines, and they worked without noise. The engines had to be made with great care, and that was, perhaps, one reason why they had not been universally adopted. One of the essential features of an engine of that description was the necessity for round shafts. If the shaft were not round it would be sure to fail. The bearing-surfaces had been designed so as to reduce the pressure to 120 lbs. per square inch. He would mention a matter that might be known to those who were accustomed to test the engines with dynamos, but not to the general public, namely, the excellent duty which direct-acting engines and dynamos gave out. The engines tested in the "Devastation" were designed for a pressure of only 15 lbs. of steam per square inch, giving out 186 amperes 83 volts, amounting to 17·4 HP. electrically. The engine indicated 22·2 HP., thus showing a duty of 77 per cent. It appeared to him that manufacturers were within measurable distance of being able to make a good direct-acting dynamo to work at reasonable speeds, so as to satisfy all parties, and to give an excellent return for the power expended. Mr. Allen.

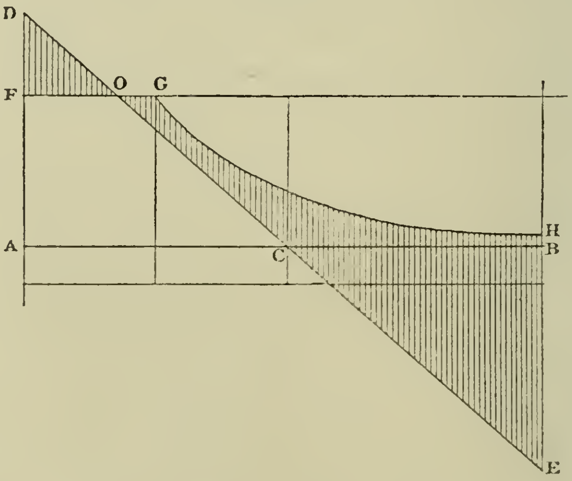
Mr. Alb. Kapteyn had been requested by Messrs. Alley and MacLellan, as the builders for this country of the Westinghouse engine, to represent them in the discussion. The general construction of these engines was undoubtedly known to many of those present. Several Westinghouse engines had been constantly in use at the International Inventions Exhibition in connection with the electric-light plant. Mr. Kapteyn.

These engines, although working under very trying circumstances, had given the highest satisfaction, and had, so to say, been running for the nine months during which the Exhibition had been open, without a hitch. A further proof that they were attracting a good deal of attention, especially in the United States, was the fact that, although comparatively recent, they were in operation at the present day already to the extent of over 30,000 HP. Mr. Imray's Paper dealt with general principles, and more particularly with the influence of the inertia of the moving masses on the smooth running, and he was afraid that if the discussion proceeded upon the question of arrangement of details, no very clear conception would be formed of the general question

Mr. Kapteyn. which the Author had in view. In the Paper, rotary-engines were compared with reciprocating-engines and the important influence of the inertia of the masses on the smooth running had been illustrated.

The third conclusion of the Author, that single action was to be preferred in high-speed motors in order to avoid the shocks resulting from alternations of strain at the working-joints, was no doubt a capital point; but he must draw attention to the fact, that simply letting the steam act on one side of the piston did not overcome all the shocks, resulting from alternation of strain at the working-joints, as it might be imagined from the wording of this conclusion. Although he agreed with most of the views expressed in the Paper, he must express regret that Mr. Imray had not worked out the points somewhat more fully, and he was afraid that some members might be led to misinterpret his views. For instance, Fig. 1 and Fig. 3 represented diagrams of the steam-pressure in an engine-cylinder, which he would call the "steam-line," and there was also given a diagram of the pressures necessary for accelerating parts, which he would call therefore the "acceleration-line." The example given by the Author, as a combination of these two diagrams, was a very satisfactory result as far as the uniformity of the net pressure on the crank-pin was concerned; but the assumed speed was moderate, and different results would be observed at high speeds. Suppose, for instance, that in a single-acting engine the speed had been such that the pressure necessary for accelerating the parts at the beginning of the stroke had been more than the available steam-pressure AF, Fig. 17, then the inertia-line DCE would cut the steam-line in a point O, show-

Fig. 17.

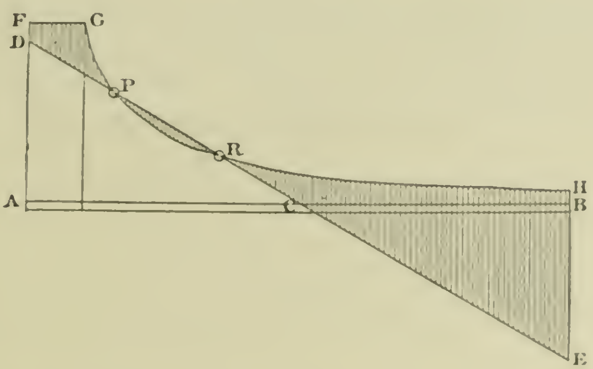


Mr. Kapteyn. ing that from F to O the fly-wheel would have to pull the piston. At the point O, however, the steam-line overtook the acceleration-line, and the piston then began to push on the crank-pin. At the point O, therefore, there was an alternation of strain at the working-joints which tended to produce detrimental shocks. To cure an engine of this defect, it would be sufficient to raise the steam-pressure so as to sufficiently cover the acceleration-line. In other words, high speed required high steam-pressure, or putting it in a different manner, all other things being equal, a condensing engine could be run at higher speed than a non-condensing engine, because condensation added about 15 lbs. to the effective steam-pressure. The defect in question might also be remedied by reducing the weight of the reciprocating-parts, because this would bring down the acceleration-line so as to be covered by the steam-line.

In the Westinghouse engine the parts had intentionally been made as light as possible. The pistons, connecting-rods, and valve-gear had all been made hollow, so as to secure a maximum of strength with a minimum of weight.

He must, however, point to worse cases than that illustrated in Fig. 17. In engines with a high degree of expansion, as in Fig. 18, it could easily occur that the acceleration-line cut the steam-line beyond the point of cut-off. In that case there were two points of intersection, P and R. At the first, P, the strain changed from + to −, and at the second, R, it altered from − to +, thus creating at both points shocks which should be avoided. A still worse case was illustrated in Fig. 19, where the acceleration-line cut the steam-line in three points O, P, and R, at each of which there was an alternation in the strain and consequent shock. There would therefore be three shocks in the course of one

FIG. 18.



Mr. Kapteyn. stroke, and this explained the rough vibration sometimes noticed in badly-designed engines.

It was, however, not sufficient for the smooth-running of an engine to consider what took place in the steam-cylinder; but it was also necessary to examine the net result at the crank-pin according to the tangent in every point. Suppose in Fig. 20, OA to be a certain position of the crank, and that at the pin A there was a net pressure AB, taken from the corrected steam-diagram. The effort AB would be resolved into AC, which was taken up by the bearing, and AD which tended to turn the engine. And, since the resistance in an engine could generally be taken as a constant quantity, the ideal engine would be that in which the

Fig. 19.

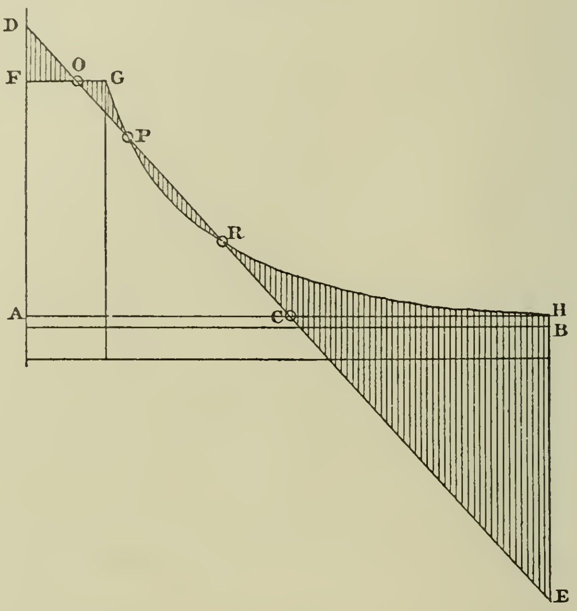
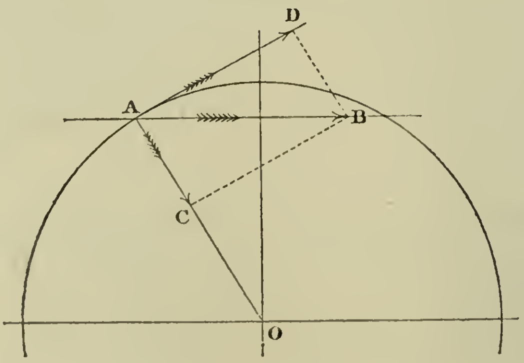


Fig. 20.



Mr. Kapteyn.

tangential effort would also be constant. This in many respects explained the great efforts which inventors were making towards producing a truly rotary-engine. However, as Mr. Imray had explained, in most rotary-engines the efforts were not constant, but to a certain degree intermittent, which therefore must be alternately greater and smaller than the uniform resistance, similarly to what occurred in reciprocating-engines. Suppose that in Fig. 21, F G H B A was a steam-line, and consider four different speeds giving the acceleration-lines AC, LC, KC, FC. Developing in Fig. 22 the path of the crank-pin and marking its positions corresponding to the positions of the piston in Fig. 21, and establishing the tangential efforts according to Fig. 20, the four diagram-lines, A L K F, would be obtained, corresponding to the four assumed speeds. Drawing in the diagram RT of the constant resistance, it was evident that the most satisfactory speed would correspond with the tangential diagram which showed the least differences with the resistance-line RT, in this case the line K. The diagram A would of course correspond to a speed zero, or in other words to a speed so low as to make the effort necessary to

Fig. 21.

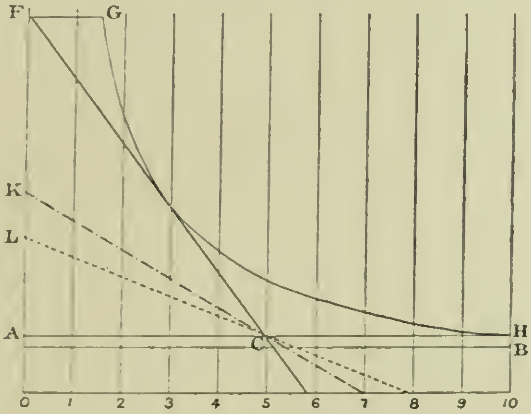
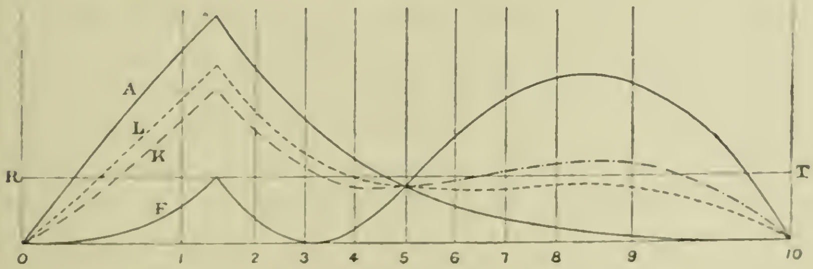


Fig. 22.



Mr. Kapteyn. accelerate the reciprocating-parts a disappearing quantity. On the other hand, the diagram-line F showed the unequal distribution of the motive power at high speed, in comparison with the pretty even distribution in the diagram-line K, and it therefore followed that by properly selecting a speed for a certain reciprocating-engine, almost perfect smooth running could be obtained.

It might be said that the rotary-engine in its abstract principle was the ideal to strive at; but so far this ideal principle had not been realized, and in regard to economy the rotary-engine stood far behind well-designed reciprocating-engines.

Mr. Siemens. Mr. Alexander Siemens said that the opening part of Mr. Kapp's Paper, in which he had stated that the theory of dynamo-machines had hitherto been treated more fully than the practice, accorded very much with his own views, and he thought that Mr. Kapp would have done well to have kept the practical part of the construction of those machines still more in view than he had done. Although he did not quite agree with the conclusions that Mr. Kapp had drawn in the second part of his Paper; he would merely refer to a Paper on the construction of dynamo-machines contributed by Dr. O. Frölich to the Electrotechnische Verein at Berlin, published in the journal of that society in March, April, and June,[1] in which the current of magnetization referred to by Mr. Kapp was very fully treated. Mr. Kapp's conclusions were favourable to the drum-armatures, and as Messrs. Siemens manufactured them, they were naturally so far satisfied with those conclusions. Mr. Kapp had discussed the various dynamo-machines in the market, and had supplied an interesting Table on the subject. He should like to compare the results tabulated with the theory propounded. He stated that for generating 1 volt in the various armatures the short drum required 16·4 inches of conductor. The only example which he had given in the Table required 18·2 inches. The cylinder-armature was stated to require from 21·8 to 26·1 inches, and Mr. Crompton's machine also required 26·2 inches. He was astonished to find that the cylinder-armature, which Mr. Kapp had constructed, required 35 inches instead of 26. It would be seen that there was a great discrepancy in the different machines, in some 74 inches being required for 1 volt, and in others only 18 inches. That bore out a favourite idea of his that since the construction of the Siemens and the Gramme dynamos, it was a waste of energy to try and find out other forms to do the same work, because the

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[1] Electrotechnische Zeitschrift, 1885, pp. 128, 139, 227.

Mr. Siemens. efficiency of those forms had been long ago fully demonstrated, and it was only necessary to bring them into a practical shape. Mr. Kapp had further stated that he had left out purposely the number of watts produced for every lb. of copper, but he had given in the Table all the necessary data, and a simple calculation showed that the length of armature-conductor per volt in inches, divided by the intensity of the current in amperes per square inch, multiplied by the weight of a cubic inch of copper, would give the amount of copper in the armature necessary to produce 1 watt. It was easy, therefore, to compare the machines mentioned in the Table with reference to the amount of copper used per watt. It was only necessary to ascertain the ratio which the length of the armature-conductor bore per volt, and the inverse ratio as to the intensity of the current. Comparing the machines, he observed that Mr. Crompton could produce a watt with $\frac{26 \cdot 2}{2{,}200}$, whereas Mr. Kapp required $\frac{35}{2{,}720}$, so that he needed nearly 50 per cent. more copper than Mr. Crompton to attain the same result. The Paper omitted the main requirement in a dynamo-machine, namely, that it should give as little trouble as possible to the customer, indeed, that was a point which was very much neglected. The endeavour of all makers was to construct a machine as light and as efficient as possible, but they overlooked the great point that it should be easily repaired, and that all the parts should be accessible.

Mr. Elwell. Mr. P. BEDFORD ELWELL would only refer to one or two points in Mr. Kapp's Paper. The Author had stated: "It is evident that of two dynamos of equal linear dimensions, but wound with wire of different gauges, that will give the greater output which has the stouter wire, because the space wasted by insulation is less." And he had given an example showing a difference in output between two machines (one having an electromotive force of 160 volts and the other of 1,920 volts) of 12,800 watts in favour of the low-tension machine. He would now take an example from actual practice. The armature of a high-tension dynamo was wound with a single layer of 0·049-inch wire covered with pure undyed silk, and gave 20 amperes at 1,000 volts, or 20,000 watts. If 200 amperes were wanted at 100 volts, it could be obtained without altering the distance between the armature-core and the field-magnets, by winding the armature with the same sized wire; but, instead of a single coil, say 3,000 feet long, it was wound with ten wires, each 300 feet long, in parallel. Thus there was obtained, Mr. Elwell. with exactly the same materials and extent of cooling-surface, an output of 200 amperes at 100 volts. But in practice, although it was the cheapest and lightest manner of making such a machine, so wide a conductor did not work well, being likely to cause both heating by unequal induction, and sparking at the commutator, so the distance between the armature-core and the field-magnets had to be greatly increased in order to introduce a thicker and narrower conductor. Now, as the strength of the field was in inverse ratio to the square of the distance, or thereabout, by doubling the latter a field of about one-quarter the strength was obtained, and, allowing fully for clearance and insulation, a conductor of three times the diameter could probably be introduced; but, as it must be four times the length it was before, there was here a loss of about 25 per cent. But that was not all. Other conditions being equal, a wire 0·049 inch in diameter would carry with safety a far more dense current than a wire of three times the diameter, as the cooling-surface was in proportion to the diameter, and not in proportion to the sectional area. Here again there was a loss with the low-electromotive-force machine, and, although much more space was occupied with insulation in the high-tension machine, it was more than balanced by the greater weight of copper, or increased size of magnets in the machine of low electromotive force. Perhaps the greater amount of skilled labour required in the construction of the former brought about an even balance in regard to cost of the two dynamos, but then there was, in the case of arc-lighting, the greater economy of lamps driven in series, and the immense saving in leads. He desired to say a word as to the drum type of armature being, according to the Author, the best of all. In order to obtain low speed, a large diameter of armature was necessary, and as soon as the diameter exceeded the length, all advantage over the ring type in length of conductor was lost, and the demand was now strongly in favour of low speeds. Again, on the drum-armature, the wires crossing and recrossing at the ends, at points where they varied very greatly in electromotive force, constituted a risk of heating and short circuit, which did not exist in the ring-armature. He must allow the statement of Mr. Kapp about the Elwell-Parker four-pole machines to remain undisputed, as the reasons which led to the adoption of this special form of four-pole dynamo would take too long to explain. But he begged to correct a mistake, owing, he was afraid, to carelessness in furnishing Mr. Kapp with sketches and data. Plate 1, Fig. 5, was not a good representation of the Elwell-Parker dynamo. The pole-pieces were too close to the side-coils, and were badly shaped. Wrought-iron pole-pieces were used in the smaller sizes. The four-pole Elwell-Parker machine mentioned in the Table, p. 151, was a specially slow-speed machine, and gave at 400 revolutions 120 volts and 300 amperes as an absolutely safe working current. The little machine certainly did carry 6,000 amperes per square inch, and ran about ten hours a day for eighteen months, being only stopped to have the armature sent to the International Inventions Exhibition last spring. Being, he believed, the first armature of the Gramme type for electric-lighting purposes, wound with a single layer of wire, it was made to do enormously heavy work in order to illustrate all the constructional improvements it possessed. He agreed with the Author in his remarks about the efficiency of dynamos. It was a great fallacy to suppose that a dynamo running at its maximum safe speed was giving double the efficiency that it would show at half the speed. It was very likely quite the reverse. One or two Elwell-Parker dynamos now at work with 16-inch armatures, showed a commercial efficiency of 88 per cent. at 400 revolutions per minute, and a periphery-speed of 1,600 feet per minute only, and it was not very likely that this efficiency would be much exceeded at a higher speed. Mr. Elwell.

Mr. Trotter. Mr. ALEXANDER P. TROTTER said he rose with some diffidence to take part in the discussion, because he had not responded as fully as he might have done to the invitation of Mr. Kapp to give particulars of the Goolden and Trotter dynamos, but he was quite willing to fill up the whole of the Table published in the Paper, giving various details of the dynamos. His difficulty had been that several recent improvements had hardly been matured. Plate 1, Fig. 8a, represented the machine which Messrs. Goolden and Trotter had been hitherto making, and which was a modified Gramme, with all the modern improvements. Only two machines had been made of the form represented in Fig. 8. The field had been copied from a dynamo manufactured by Messrs. Ganz, of Budapest. There were several drawbacks in connection with it. There was a consequent point in the upper part of the side magnets, but no doubt certain advantages were to be gained from the double effect of the inner coil.

He thought that some reference should have been made, in the first part of the Paper, to the improvements effected by Dr. Hopkinson on the Edison dynamo, and to the result of the publication of the details in the autumn of 1883. Almost all the important improvements in dynamo construction appeared to have been dated

Mr. Trotter. from that time, when makers began to put more iron in the armature, and to properly proportion the fields.

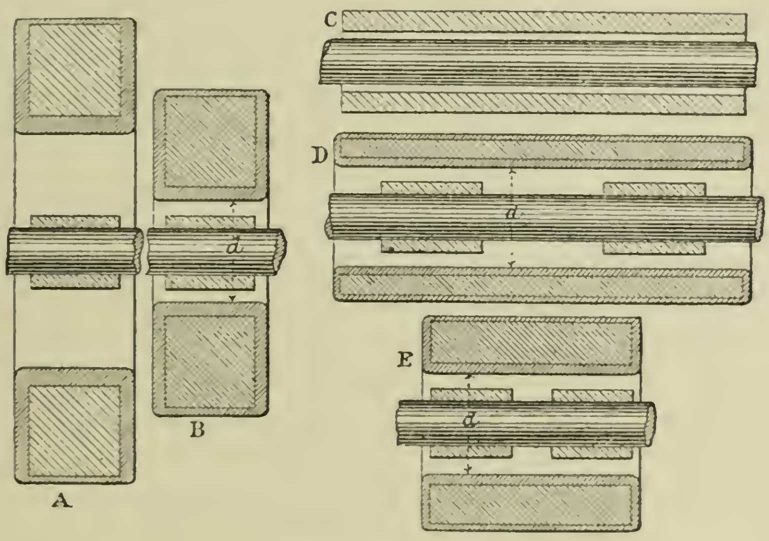
The general scientific world, as well as the engineering profession, was greatly indebted to the Author for the most important law of magnetic resistance. He did not know whether it had been published before; but it certainly had never been so clearly and publicly enunciated. It first came before his notice in a private letter from Mr. Kapp, in which he pointed out that the magnetizing force, the magnetic effect, and the magnetic resistance followed a law which very closely resembled Ohm's law, where the magnetizing force was analogous to the electromotive force, the magnetic resistance to the electrical resistance, and the magnetic effect to the current. Magnetic resistance as thus defined was a concrete quantity, which not only performed an important part in all electro-magnetic phenomena, but could now be easily measured. That threw a new light upon the construction of dynamos, especially on the relation of the field to the armature. The mode of deducing the carrying capacity of a coil, from the number of square inches necessary to dissipate heat generated by a watt, was an excellent one. For single-layer armatures he arrived at the same result, which worked out well in practice, raising the diameter of the wire to the power $\frac{3}{2}$ according to Professor Forbes' formula, and $d^{\frac{3}{2}} \times 900$ gave almost exactly 1·8 square inch per watt. Mr. Kapp had laid great stress on the driving of the wires by friction. It appeared to him, however, to be rather straining a point when engineers were told that the ordinary way of wrapping a wire round an armature was hardly sufficient, mechanically speaking, to drive it. Upon a 12-inch armature, with about 110 turns of wire upon it, and taking about 22 HP. to drive it, there was about 2 lbs. pull on each wire in ordinary working, and supposing the machine to be short-circuited, and to require, according to the Author, fifteen times that pull to be transmitted, that was to say before the engine would be pulled up or the belt thrown off, there would be about 30 lbs. to each wire. He thought that even if one wire only were properly wrapped, it would stand 30 lbs. pull without slipping, and if it did slip in the very least, it could not move enough to do any damage to the insulation before there would be an enormous strain which would effectually prevent further motion. He rather grudged giving up space on the circumference of the armature for the necessary projections.

He exhibited some diagrams illustrating a criticism which he wished to offer upon the Author's Table of inches per volt. Mr.

Mr. Trotter.

Kapp had stated that the depth of the core should be one-fifth of the diameter of the armature; or at all events he assumed that this was a practical proportion. He saw no possibility of making such an armature as that given at No. 3, "cylinder, long." The mode in which he would set about the design was to allow a minimum space inside the armature, such space being required for the shaft, the hub, and the wire, and for ventilation. Taking a core of a total section of 40 square inches, his engineer, Mr. H. W. Ravenshaw, had calculated the dimensions of the cylinder-armatures described as "very long in comparison to the diameter," with 21·8 inches per volt, and a cylinder "length equal to the diameter, rectangular core 5 : 1," with 26·1 inches per volt, and a "disk length equal to the radial depth, square core." This last was taken as a cylinder; running at 3,000 feet per minute, and considering the wire on the outer side only, this gave 44 inches per volt. Figs. 23, A, C and E, represented the sections of these armatures, assuming "that the radial depth of core in the cylinder and disk is one-fifth of the diameter." This assumption could not be reconciled with the proportions given, as was evident from A and C, where A was an armature with a core 4·5 inches square, and C was an armature of similar total section, with a core 20 inches long, by 1 inch deep, still maintaining the proportion of 5 : 1. There was no room for the wire, much less for the hub or for ventilation, keeping a 2½-inch shaft throughout. If he had to design an armature with a square core, he should first

Figs. 23.



Scale 1 inch = 1 foot.

Mr. Trotter. settle the minimum space required for the wire, for the hub, and for ventilation (in this case 5 inches), and would make an armature as in B, the weight being reduced 42 per cent., the useless space inside avoided, the electrical resistance greatly reduced, and the inches per volt diminished by 32 per cent. Taking the other extreme case, if a core of 20 : 1 were demanded, he should proceed in the same way as with D, where the construction became possible, though not to be recommended. E showed the mean case, and this corresponded exactly with the Goolden-Trotter armature, and though notched armatures were being adopted, almost similar proportions held good. This proportion was not arrived at by considerations of length of conductor per volt, but by making the core as short as possible (to keep down the electrical resistance), consistent with the maintenance of enough surface of pole-piece for good magnetic resistance.

The accompanying Table gave the details of each armature, A, C and E being constructed according to Mr. Kapp's assumptions, and B, D and E according to the practical requirements. The relations of the two systems were clearly represented by curves (Fig. 24). The ordinates were inches per volt, and the abscissas were the ratio of the length of the core to the thickness. Mr. Kapp's method gave a decrease of length, at first rapid and then gradual, the best result being attained at an enormous speed. The practical method showed first a decrease and then an increase. The best point was about 1 : 3. The ratio of the Goolden-Trotter armature was from 1 : 4 to 1 : 5. This had only 1 inch per volt more than the best possible, but had a considerably smaller magnetic resistance. The following formula had been calculated by Mr. Ravenshaw for finding the diameter of an armature; it

—	Core.		Diameter inside wire = *d*.	Speed per minute.	Inches per volt.
	Length to thickness.	Diameter outside.			
		Inches.	Inches.		
A	1:1	22·5	11·5	510	44
B	1:1	15·4	5	750	30
C	20:1	5	2	2,290	22·8
D	20:1	7·75	5	1,475	35·5
E	5:1	10	5	1,155	26

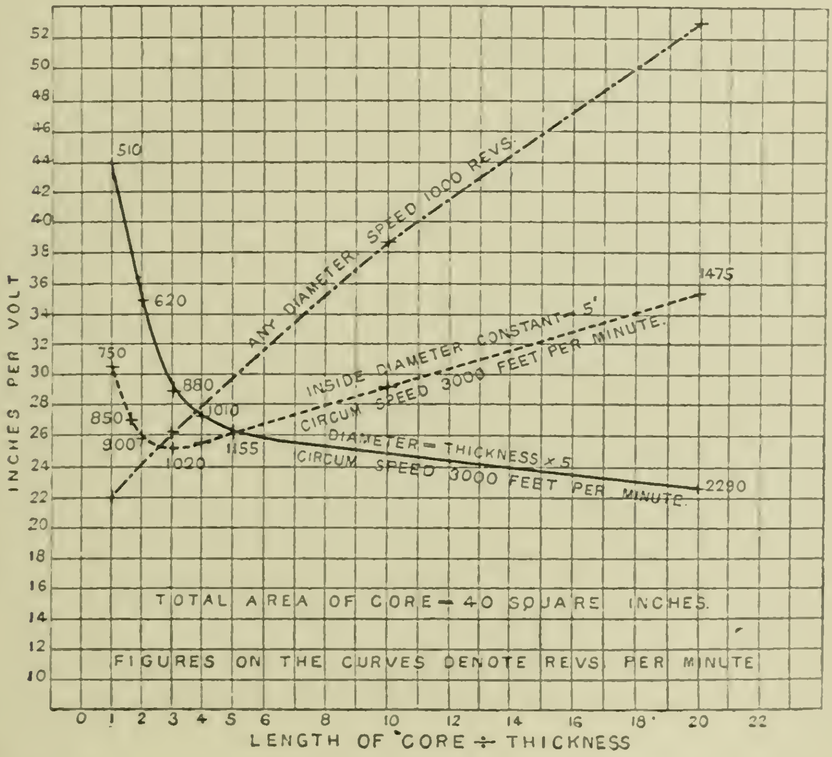
Circumferential speed, 3,000 feet per minute.
Total area of section of core, 40 square inches.

might appear clumsy to a mathematician, but it was in a convenient shape for practical use. Mr. Trotter.

$$G = 2\left(K + \sqrt{\frac{d^2}{4} + 2\,H\,K + 2\,K^2}\right)$$

Where d = the necessary inside diameter of the complete armature to allow for ventilating-space, hub and shaft; H = thickness of core; K = outside depth of wire; and G = diameter inside the core.

Fig. 24.



Mr. Esson. Mr. W. B. Esson stated that Plate 1, Fig. 7, was scarcely a correct representation of the latest type of machine made by Messrs. Paterson and Cooper. In the latest type the magnets were of wrought-iron, and there were no interior lips projecting inside and forming extensions to the polar-surfaces. The magnets were simply hollowed out to receive the armature, and since the brackets supporting the spindle were attached to the magnets on each side, the bars could be almost completely cut through without sacrificing the rigidity of the machine. Their plan was to cut the bar three-fourths of its thickness through. A 6-inch bar would be cut until the thickness at the crown was only $1\frac{1}{2}$ inch. He

Mr. Esson. wished to direct attention to Mr. Kapp's figures as to the number of inches of wire required per volt. On his assumptions as to dimensions the figures were correct, but he thought that the depth which he had given for the disk-armature was by no means the best proportion. If the radial depth were 0·3 of the diameter instead of 0·2, and if the length were equal to the radial depth—that was, if its section were of square form—it would, he thought, be found that this armature, instead of giving 26·3 inches per volt, would give 19·7 inches at a circumferential speed of 4,500 feet instead of 5,000 as stated by Mr. Kapp.

With reference to the Author's remarks upon toothed armatures, the makers of machines having Pacinotti rings had come under Mr. Kapp's criticism, and Messrs. Paterson and Cooper happened to be amongst the few makers who constructed armatures of that character. Mr. Kapp had stated that no amount of lamination of the pole-pieces would keep down the heat with an armature so constructed. That was true, but if instead of laminating the pole-pieces the surface of the armature itself were divided, there being instead of a few teeth with large spaces between them a great many teeth with small spaces, and, if in addition a clearance between the teeth and the polar-surface equal to 0·35 of the width of the spaces were given, there was no heating of the polar-surfaces in the longest runs. In questions of applied science it was generally assumed that when an effect which had appeared for some time presently disappeared, the cause which had produced it had also disappeared, and he thought that dynamo-machines should form no exception to that rule. Foucault currents were always accompanied by heating, of which they were the cause. If with one armature the pole-pieces were found to get very hot, and if by the substitution of another there was no heating, he thought the logical conclusion was, that in the latter case there were no Foucault currents. Mr. Kapp had bewailed the uncertainty of dynamo-makers in regard to the proportions of the machines. That, he thought, was to a great extent due to the fact that dynamo-makers were not fully agreed as to the kind of machine they had to make. If they had to make a machine which gave the highest possible efficiency, and the highest number of watts per lb. of copper employed in its construction, without reference to the weight, that was a very different thing from a machine which gave a maximum number of watts for a minimum weight of machine; and until there was some general conclusion as to what was to be made, engineers could hardly expect much more agreement amongst the makers. Again, in most questions

Mr. Esson. of applied science, there was a considerable difference of opinion as to the way in which particular results ought to be attained, even when there was complete agreement as to the desired result, which there was not in this instance. With reference to the engine mentioned by Mr. Kapp as having run successfully at the International Inventions Exhibition, he did not think that that engine showed an exceptionally high efficiency in the way of continuous running at high speed. Some engines supplied by Messrs. Gwynne far exceeded in speed Messrs. W. H. Allen and Co.'s engine. He did not see why the piston-speed should be limited to 283 feet per minute, or even 600 feet per minute, if the engine were properly designed for the work it had to do.

Mr. Mordey. Mr. W. M. Mordey remarked that Mr. Kapp had given some useful information about the number of poles and magnets forming the field-magnet systems of dynamos. While agreeing with most of Mr. Kapp's conclusions, he wished to point out that in some cases other circumstances than those mentioned had presented themselves for consideration. As an example, he would endeavour to describe and explain the circumstances that led to the development of the "Victoria" dynamo from the original Schuckert machine.

The Schuckert dynamo consisted, as was doubtless well known, of a flat ring- or disk-armature, moving in a field which presented two poles to the armature. The magnets had generally been made in four parts, but nevertheless it was simply a two-pole dynamo. It worked fairly well, but not as well as some other machines, and there was this peculiarity about the collection of the current—a considerable movement of the brushes on either side of the ordinary diameter of collection might be made without more than very slightly affecting the current and electromotive force. As to the collection of current, it was scarcely made perceptibly worse by such movements. The ordinary working position of the brushes was between 30° and 40° in advance of what was sometimes called the theoretical neutral line.

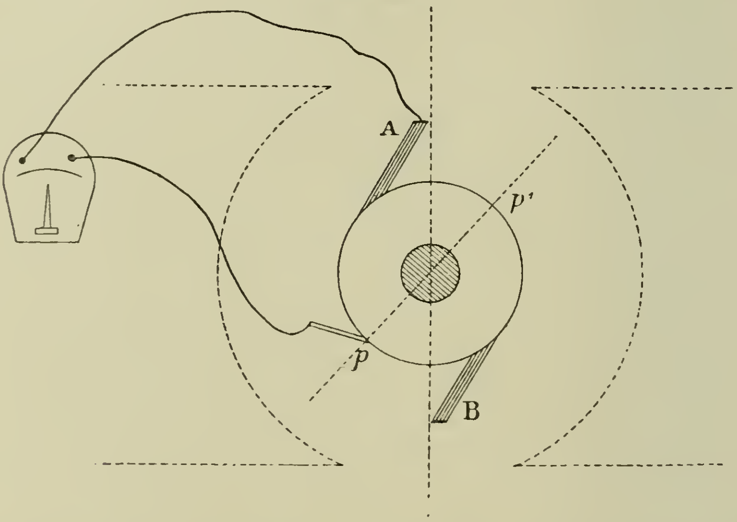
Wishing to examine the character of the inductions taking place in the armature, he had devised a simple method of doing so, which he might be allowed to describe, as it had proved very useful in practice.

When the dynamo was running, and supplying current as usual, one terminal of a voltmeter, preferably an Ayrton and Perry instrument, which indicated the direction as well as the amount of the electromotive force, was connected to one of the brushes, as shown at A (Fig. 25). The other terminal of the volt-

Mr. Mordey. meter was connected to a small brush, which might be placed in contact with the commutator at any desired point.

Now, under ordinary circumstances, if the small brush were moved over the commutator of a dynamo, beginning at one of the main brushes, a gradually increasing deflection would be observed, always in one direction, as the small brush was made to approach the second main brush. When this second brush was reached, the voltmeter would of course indicate simply the difference of potential between the two brushes. The manner in which the deflection had increased would, however, have shown the part played by each portion of the armature in contributing to the total effect.

Fig. 25.



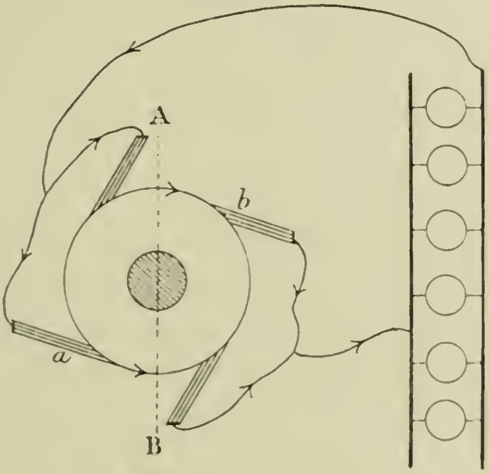
In applying this method to the Schuckert machine, he found that the voltmeter indicated a gradual rise of potential in front of the brush for about 30°, but the deflection was in the wrong direction; that was to say, it indicated an electromotive force contrary to that which existed between the main brushes A and B. On continuing the motion of the small brush, the deflection gradually fell, until at a point p, about 100° in front of the brush A, the voltmeter gave no deflection, indicating that the two points spanned by the voltmeter were equipotential. On carrying the voltmeter contact-brush still further forward, past p in the direction of B, an indication in the opposite direction was given, rising quickly till B was reached, and showing that the whole of the useful electromotive force of the machine was developed in the small portion of the ring included between p and B, and, of course, in the corresponding region on the other half of the ring between p' and A.

Mr. Mordey.

This abnormal character of the induction or reaction in the armature made it possible to carry out one or two peculiar experiments, which he would venture to describe.

Every electrician knew that if a wire was made to touch one of the brushes of a dynamo, and any point of the commutator at a distance from the brush, a severe short-circuiting was the immediate result. But in this case it was possible to permanently connect two distant points of the commutator, because there was no difference of potential between those parts, and this without in any way affecting detrimentally the output of the machine, although a large part of the armature was apparently short-circuited. He had made such an experiment. A Schuckert machine was provided with four brushes (Fig. 26). A and B were the ordinary brushes. Two other brushes, *a* and *b*, were placed on the commutator at the point found by the voltmeter to be equipotential respectively to A and B. These four brushes were connected in pairs A *a*, B *b*, by short stout conductors, each pair being used then as a common brush, the machine the while working normally on its ordinary circuit. If the course of the current were examined under these conditions, as shown by the arrows, it would be seen that the whole of the portions of the armature included between the connected brushes were absolutely idle. There were two equal and opposite electromotive forces in each such portion, and as it was short-circuited by a stout conductor, practically no current could flow through the inclosed portion from the other parts of the armature; and no current being generated in it, on account of the equalization of the opposing forces, it was absolutely idle. The whole of the work was done in the small portion of the

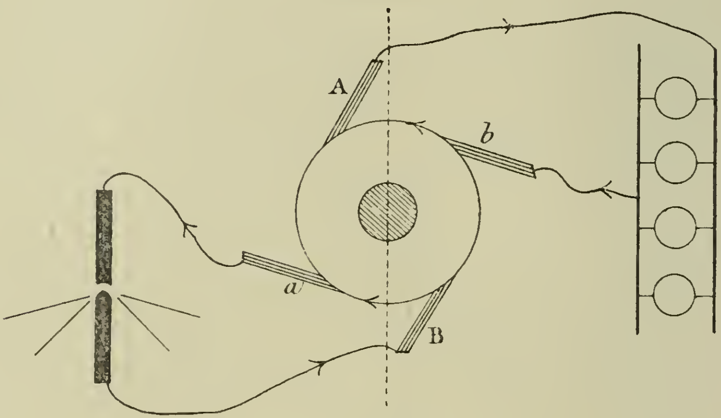
Fig. 26.



Mr. Mordey. armature between the pairs of brushes. The effect on the resistance of the armature was to considerably reduce it by removing from the circuit nearly two-thirds of the armature-conductor. As a matter of fact, there seemed no reason to doubt that every two-pole Schuckert dynamo ought to be worked with four brushes on its commutator, instead of two.

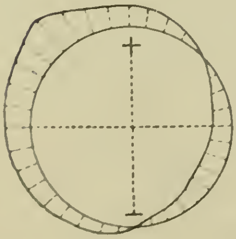
He had tried another arrangement, shown in Fig. 27. The connections between the equipotential brushes were removed, and each pair *a* B, *b* A, was used separately and independently, operating two distinct circuits. For instance, arc lamps might be used on one, and incandescent lamps on the other, the fields being excited from either. If the machine were separately excited, the current in either circuit could be varied without affecting the other; or one circuit could be in operation and not the other. It would be seen that in this case also, no current whatever traversed the armature between the equipotential brushes, and that the resistance of these portions was removed from the circuit.

Fig. 27.



He had shown the result of the examination of the potentials round the commutator to Professor Silvanus Thompson, who suggested that they should be plotted as curves, either round a circle or along a straight line representing a commutator. Figs. 28 and 29 represented the result for the Schuckert machine; while Figs. 30 and 31 indicated the distribution of potentials when the excessive reactions were eliminated. An examination of Fig. 29 showed that the useful portions of the armature were *a a*, while the useless portions were *b b*. It might be supposed at first that the brushes were wrongly placed, and that they

Fig. 28.



Mr. Mordey.

should be at the positions corresponding to the greatest difference of potential; that was, at the top and bottom bends of the curve, but in such positions the machine would not excite. Professor Thompson had further investigated this question of commutator potential, and had arrived at some interesting results. It might be pointed out that curves of this kind served the useful purpose of showing exactly what was going on inside an armature, and in this respect supplied information, in a slight degree not unlike that which in the case of the steam-engine was furnished by indicator-diagrams.

Having discovered the character of the faults in the Schuckert machine, it was necessary to improve it by getting rid of them. The explanation he had given showed that there were large portions of the armature useless or worse than useless. These portions were rendered useful and productive by increasing the number of the poles from two to four. Thus four useful portions were obtained instead of two, and the opposing inductions, which were the cause of the low "activity" of the original machine, were reduced and finally eliminated. The same winding remaining on a given armature, it would be evident that double the current could be drawn with exactly the same loss in the wire as before, as the same current flowed through each portion of the wire. Without increase of speed, it was found easy to more than double the output of the machine, and at the same time to increase its efficiency, both electrical and commercial. He had mentioned these matters by

FIG. 29.

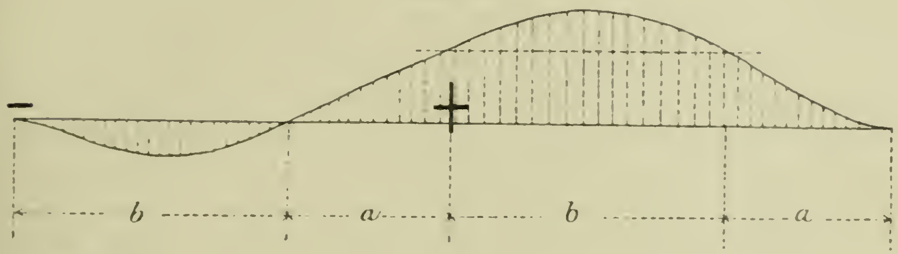


FIG. 30.

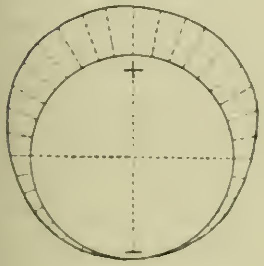
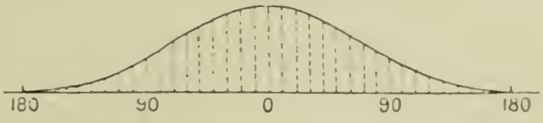


FIG. 31.



Mr. Mordey. way of supplementing the part of Mr. Kapp's Paper which dealt with the proper or best number of poles. He would not enlarge upon the more recent improvements introduced by the Brush Corporation into the Victoria dynamo; several of them had been touched upon by Mr. Kapp.

As to the question of speed, he supposed that at present no one recommended such high speeds for dynamos as were common some time ago; but he thought that Mr. Kapp was riding his hobby of low speeds rather too hard. It was unfair to ask all makers to keep their speeds down to that of a particular engine that happened to suit Mr. Kapp's dynamos. Mr. Kapp had stated that the loss in a dynamo was proportional to the square of its speed. Mr. Mordey admitted that some of the losses in a dynamo were proportional to the square of the speed, and even increased more quickly still; but it was not correct to say that a dynamo, that, at a certain moderately low or moderately high speed, would do a certain amount of work, could be made to do the same work more economically at a very low speed. The machine would, according to Mr. Kapp, require to be made larger in order to do the work at the low speed. It was only in the case of a dynamo being lightly worked, that it ought to have its speed reduced without change in size. He was not prepared to admit that the law mentioned of the increase of the losses with the square of the speed was correct, because in the case of low-speed machines there was a larger armature—a greater mass of iron—revolving in a field of the same or greater strength. Therefore the loss in eddy-currents and in other ways, with a large machine at a low speed, might perhaps be as great as in a smaller dynamo running at a rather higher speed. Mr. Kapp's statement no doubt applied to a given dynamo at different speeds, but not to a dynamo required to do a given amount of work. If very low-speed dynamos were really necessary, they could be made easily enough; as had been shown long ago by the Brush Corporation and others; but such speeds meant increased size and cost, and Mr. Mordey thought there were plenty of obstacles in the way of the extensive introduction of dynamos, without setting up an artificial and unnecessary standard of low speeds, with the accompanying increase of cost to the consumer.

Mr. Fricker. Mr. G. C. Fricker observed that attention had been bestowed in Mr. Kapp's Paper on certain mechanical details which were very necessary to good working. He thought with Mr. Siemens that all parts of the machine should be readily accessible, and it was also a feature in favour of the disk-ring armatures that the

Mr. Fricker.

coils on the armature could be easily examined, repaired, or removed. The next improvement was the multi-polar principle in disk-machines. The effect of multiplying poles on an armature was to increase the magnetic-field, yet keeping the density of the lines of force constant in the armature-core itself. In regard to theoretical considerations Mr. Kapp had given the formula

$$E a = z \cdot N t \cdot n \cdot 10^{-6}$$

He had then demonstrated its truth from a fundamental formula. Working from it he gave the relative lengths of wire for the production of 1 volt with a density of 20 lines in armatures of various types. The Gülcher Electric Light and Power Company had lately adopted a circular core, in order to get a maximum electromotive force with a given length of wire on a machine of a given weight, and he was therefore rather astonished to find that Mr. Kapp had put the circular core on the same terms as a square core. But his figures were correct, assuming that the radial depth was the same in each case. He hardly thought, however, that that would be quite fair, because the square core would evidently entail a heavier machine than the circular core. Taking machines of the same weight, and calculating for 1 volt with one turn of wire, it would be found that the circular core would require 12½ per cent. less wire than the square core.

Prof. Forbes.

Professor GEORGE FORBES said that Mr. Kapp, in the first part of his Paper, had dealt with the general nature of the improvement of late years in dynamo-machines. Those improvements had undoubtedly been of two classes; one electrical, or rather magnetic, and the other purely mechanical. The electrical defects of machines six or eight years ago were very evident to all electricians. He was sure that many of those present had been surprised about the time of the Paris Exhibition at seeing the number of machines of the Gramme type, each having about half-a-dozen magnetic legs, and each with a separate coil of wire. The great Edison machine, for example, had eight such legs separately coiled. Of course Dr. Hopkinson was right in inducing the Edison company to adopt a single pair of a circular or square section. The chief improvements, besides the shape of the field-magnets, had all been mentioned by Mr. Kapp, and Professor Forbes, in spite of what Mr. Trotter had said, laid great stress upon the feature, brought so prominently forward by Mr. Kapp, of transmitting the power to the wires by direct driving, and not by friction. He believed that had been a decided advance. Another great improvement of which Mr. Kapp had spoken lightly, but with which

Prof. Forbes. he had been largely connected, was that of the thorough ventilation of the armatures. Formerly armature-cores were always made of wire; now a very large number of them were formed of disks or tape. The object of having either the wire or disk was to prevent the formation of what were called eddying-currents; but when disks were used, the direction in which the eddying-currents had a tendency to be created was perpendicular to the disk. The disk had the further advantage of allowing the magnetic induction to go more freely through it than across the sections of a bundle of wire. He did not know, however, whether that advantage was really so great as it appeared theoretically to be. The fact that Mr. Kapp made his own dynamo of wire, instead of disks, seemed to lead to the conclusion that practically the advantage had not been found so great as might have been supposed. Then again, by reducing the number of layers of wire upon the armature, and by attention to the improvements in the construction of commutators and brushes, the sparking had been almost entirely abolished. Every one in practical work would know that that was one of the greatest improvements effected during the last few years.

The second part of Mr. Kapp's Paper dealt with theoretical considerations, but perhaps he ought to have called them practical considerations, because theory had always to be applied to practice. How any one could design a machine without theoretical considerations of that kind was to him a mystery, but he believed it had been sometimes attempted. With reference to the formula given by Mr. Kapp, he desired strongly to protest against the introduction of the letter z. Electricians were continually troubled by differences in nomenclature, and Mr. Kapp had introduced a new term which had no homogeneity with any system of units that had ever been invented. He was sorry to see it in print, because it would assuredly lead others to use the letter z. He would not say much about the relative values given by Mr. Kapp in inches per volt for the different types of machine. He would simply assert that although the figures might be to a certain extent in the order of merit of the different machines, they did not actually represent the proportionate merit of the different machines. The important practical part of Mr. Kapp's Paper was that in which he had dealt with the magnetization produced in the armature by means of the field-magnet coils. He had given a formula which had been arrived at empirically, and it was certainly a very startling one to a mathematician. It would have been thought that such a formula would not be likely to represent anything; that in all probability a constant would have been wanting somewhere; but that was not the case. It was remarkable that such a formula should have been arrived at simply by testing the machines. The type of formula was an old one. Its object was to express the amount of magnetism in the iron in terms of two quantities, the numerator being the magnetizing force, the number of ampere turns on field-magnets, the denominator being what Mr. Kapp called magnetic resistance, to be measured in the same manner as electrical resistance. Thus he had got magnetic induction in terms of magnetizing force divided by resistance, just as electric current was given by electromotive force divided by resistance. That manner of expressing magnetization was originally due to Faraday, who imagined a straight magnetizing bar of iron as being analogous to an electrical battery, with insulated wires going to points corresponding with magnetic-poles, and the points exposed; the whole being dipped into water, there would then be a circulation of electric current through the water and battery. Such was the wonderful mathematical nature of Faraday's mind, that he actually saw that the intensity of the currents in different parts of the field was analogous to the intensity of magnetization in different parts of the field when the rod of iron was placed there. It was a most remarkable piece of insight, without the employment of mathematical symbols. The same principle was employed by Professor Rowland in his celebrated research upon magnetic induction, in which he was calculating the induction in a bar and in a ring of iron on the same principle as that of Ohm's law. Finally, Mr. Bosanquet, in the "Philosophical Magazine,"[1] drew attention to the manner in which that formula might be applied. At that time it was manifest that a formula of that sort would represent magnetization. He had taken the trouble to compare the magnetization got out in that way, directly measuring the resistance and measuring the magnetic force in the proper manner, and it might be interesting to show the results of the theory, and to compare them with Mr. Kapp's empirical formula, because the similarity was very remarkable, and the difference was very instructive. The theoretical formula was— Prof. Forbes.

$$\mathrm{N} = \frac{3 \cdot 12 \times \mathrm{P}}{\frac{2\,\delta}{\lambda\,b} + \frac{l}{\mu_1\,a\,b} + \frac{\mathrm{L}}{\mu_2\,\mathrm{A\,B}}} \cdot \frac{\mathrm{A\,B}}{\mathrm{A'\,B'}}$$

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[1] Vol. xv. Fifth Series, 1883, p. 205.

Prof. Forbes. where N was the number of lines of force in C.G.S. units. Mr. Kapp objected to the introduction of a large number of constants, but he could not do without them. There were only two constants μ_1 and μ_2 which were not in Mr. Kapp's formula, and they referred to the intensity of magnetization in the iron when there was a certain amount of magnetizing force acting on it. They could be very easily got at; in fact, Tables of the values had been given by Professor Rowland. A B, Mr. Kapp had explained, was the section of the field-magnets. Now field-magnets were surrounded by field-wires to a certain extent, and A′ B′ was the average section, the mean area included by those wires. Taking the strength of magnetization, which Mr. Kapp had shown to be in his dynamo in the armature and in the field-magnets, he knew what the values of μ_1 and μ_2 would be, and he would actually reduce them to the form which would suit Mr. Kapp's dynamo. The formula became—

$$N = \frac{3{,}000\ P}{1{,}440\ \frac{2\ \delta}{\lambda\ b} + \frac{l}{a\ b} + 0{\cdot}6\ \frac{L}{A\ B}}$$

That was exactly the same as Mr. Kapp's with one exception. He had 2 instead of + 0·6. It was a most remarkable coincidence that by a simple empirical examination he should have arrived at a formula so completely in accordance with theory.

He wished to say a few words as to the remarks of Mr. Kapp in reference to passing from the smaller to the larger, or from the larger to the smaller machine—a point which he did not think had been treated fairly. In the first place the two machines were not one the model of the other. The dimensions in the one were not the linear multiple of those of the other. Thus Mr. Kapp multiplied the dimensions of the wire on the armature into the power of $\frac{3}{2}$ of linear dimensions, and the field-magnets only to the power of 1. He objected to Mr. Kapp's using different strengths of field in the machines. He had worked up to getting the heat of the surfaces of the coils equal, and he ought also to have had the field equal. He had applied the arguments, such as they were, to two different purposes. The first purpose was to ascertain the price of the machines as they changed in dimensions; and then he submitted a Table to show how he had calculated the relative prices, giving as a proof the fact that they agreed with the makers' prices. If that was the object of the whole investigation he would rather go to the makers and find out the prices there. The theory was also applied to prove that a small machine

Prof. Forbes.

would not work properly; but Mr. Kapp had made the smaller machines on wrong principles; he had made the clearance between the armature of the pole-pieces much too great. He had stated that the clearance could not be in proportion to the linear dimensions. It could be, if the workman were forced to do his duty. It was simply carelessness that prevented a model being made of the same relative dimensions as a larger machine. The clearance could be made proportional in the two cases. The danger arising from the one or the other was equal. He meant that, if a clearance was made proportionately to the linear dimensions, if it was safe to run the one it was safe to run the other. But Mr. Kapp had not applied the theory to the far more important matter of finding out what could be got from machines of a larger size than those hitherto constructed. It was impossible from the way in which he had treated the subject to get that information, because the one machine was not made the model of the other. If the linear dimensions of a machine were increased in any proportion, and the speed increased in inverse proportion to the power of $\frac{3}{2}$ of linear dimensions, each of the two machines would work perfectly whether shunt-wound, series-wound or compound-wound, and the output would be greatest in proportion to its weight for the smaller machine. One very great improvement that might be expected in the construction of dynamo-machines was that makers would in future specify their iron for magnetic qualities. It was quite easy to test magnetic qualities, but there might be a difficulty in getting people to supply iron to a magnetic specification. The same difficulty had been experienced in former days in getting people to supply copper for conductivity specification, but that was soon overcome when the demand was sufficient to make it worth while; and so it would be in the case of iron. The Author had spoken of the different types of machines generally in use, but he ought to have said in use at the International Inventions Exhibition at South Kensington; those used on the Continent and in America having been completely ignored. He had referred to the Brush and the Houston-Thomson, but he had ignored the Hochhausen which was largely adopted in America, and also the Weston, one of the best arc machines in existence. Notwithstanding there were so many machines, still there was the same fault in them, and it might be asked why it was that people continued to make flat field-magnets, instead of circular ones, or such as those suggested by Dr. Hopkinson for the Edison dynamo. It was well known that the latter were more satisfactory, and that less copper would be wanted in winding them if only two legs were used to

Prof. Forbes. the field-magnet instead of four. He wished to point out a mistake (which he had often seen made before) in the Author's statement as to the great advantage of the Pacinotti type of armature. He first saw it expounded in the Cantor lectures by Professor Sylvanus Thomson. He made an experiment with a Gramme-ring on which he wound a single coil, then he put in Pacinotti pieces of iron where other coils might have been. He turned it round and found that he had got twice the electromotive force through that one coil when the Pacinotti iron was in as compared with the amount when it was out. He had overlooked the fact that if he had taken out the Pacinotti iron he might have put in as many additional coils of wire, and so have doubled the electromotive force in that way.

Prof. Ayrton. Professor W. E. AYRTON said that Mr. Kapp's Paper was to him of great interest, because it had placed the results of experience in a form both practical and suitable for electrical engineers to use in the designing of dynamos. Considering the great number of dynamos made during the last few years, it might appear at first sight rather strange that they had not at their fingers' ends the information that would enable them to foretell the output of a dynamo, when they had decided upon its dimensions. But, as a matter of fact, there were two elements of great vagueness in connection with the construction and designing of dynamos: one, what was the specific magnetic resistance of different kinds of iron; and the other, what was the variation of the magnetic resistance in a given mass of iron with its shape. Many attempts had been made to express the amount of magnetization produced by a given magnetizing force in terms of magnetic resistance, just as electricians were accustomed to express the strength of an electric current produced by a given electromotive force in terms of electrical resistance by Ohm's law; and he thought that Mr. Kapp's attempt was perhaps one of the most happy. The difficulty of deciding on the magnetic resistance of a given circuit, in terms of its dimension, was much the same as endeavouring to say what was the electric resistance from one point to another of a copper wire of irregular shape, immersed in a conducting liquid, and that, as electricians knew, was a problem of considerable difficulty, because in such a case the lines of flow followed not merely the copper wire but also the liquid; whereas, if the wire were in a perfectly-insulating fluid such as air (like a telegraph wire, for example), the lines of flow would merely follow the wire, and the problem would be a simple one. In spite, therefore, of the dispiriting advice given by Mr. Siemens, not to trouble about attempting to improve dynamos, he

Prof. Ayrton.

thought that Mr. Kapp's work was certainly good, and in the right direction. It was, of course, true that since dynamos had attained an efficiency of something like 90 per cent., and as 100 per cent. was the maximum, there was not much room for improving the efficiency, but he knew of no mathematical reason for connecting the power of a dynamo with its present weight. Electricians now had something like 1 HP. per 100 lbs. dead weight, and he knew of no mathematical reason to prevent their getting 1 electric HP. for 30 or 40 lbs. dead weight; and therefore efforts to improve dynamos were, in his opinion, by no means useless. Perhaps one of the most valuable parts of Mr. Kapp's Paper, was his attempt to express the magnetization induced by a given magnetizing force in terms of magnetic resistance; but when Professor Ayrton came to examine the formula, given at page 137, he was not altogether in accord with the Author. Mr. Kapp wished to see how the magnetic resistance was altered when a horse-shoe magnet with two limbs was substituted for one with one limb only, and he concluded that if two limbs were put in the place of one, the magnetic resistance of the air would be doubled, and also that of the armature, while the resistance of the field-magnet would remain as before. Professor Ayrton should have thought that the magnetic resistance of the air would be the same, and also of the armature, and that the resistance of the field of the magnet would be halved by putting a horse-shoe magnet with two limbs. There was, however, a more important point in regard to which he differed from Mr. Kapp, namely in his expression for the resistance of air. He had put forward a formula, in which he stated that the resistance of a given layer of air was 1,400 into twice the thickness, divided by the area of the armature-core occupied by the iron. In other words, Mr. Kapp concluded (he did not know whether it was simply from general reasoning or from experiment) that the magnetic resistance of a layer of air separating two magnetic-poles was proportional to the thickness. But Professor Ayrton should rather have concluded *à priori* that the magnetic resistance of a layer of air was not proportional to the thickness, and that it increased much more rapidly than thickness at first, and afterwards slowly with the thickness; in fact, that the resistance of a layer of air, of variable thickness separating two magnetic-poles, would follow somewhat the curve obtained several years ago by Professor Perry and himself, for the electric resistance of air. In that case they found the curve something like the characteristic curve of a dynamo, horizontal distances representing the distance between the electrodes, and vertical

Prof. Ayrton. distances the electrical resistance of the air between these electrodes. Indeed, that general analogy had led him to make an investigation on the magnetic resistance of air. He would not attempt to explain the details, but he might state generally that the result was that the magnetic resistance of air was not proportional to thickness, but followed a curve of the kind he had described. In considering the magnetic resistance of the air they had to carefully distinguish between the resistance of a tube of air of given section, and the resistance of all the air between pole-pieces of given section. The former might be accurately proportional to the length of the tube, but the latter—which was really what was of importance in dynamos—was not, he thought, proportional to the distance between the face of the pole of the field-magnet and the iron core of the armature, as assumed by Mr. Kapp. This consideration of the resistance of the air was of great importance when the relative advantages and disadvantages of the Pacinotti armature were compared with the Gramme. The Pacinotti armature had certain protruding pieces of iron, and its advantage was that those pieces of iron, coming close to the pole-pieces of the magnets, produced a much less air-resistance—a much thinner layer of air to produce resistance; and if, as he had said, the magnetic resistance increased rapidly at first, and then more slowly, it followed that, by bringing the iron much nearer, a much stronger field would be obtained than by winding the armature simply with iron in accordance with the Gramme mode of winding. Professor Forbes had summed up the question of the relative advantages by saying that there seemed to be no advantage in the Pacinotti armature, because, although only half the electromotive force could be got with a Gramme armature without the protruding pieces of iron, that could be got with the Pacinotti armature with the same amount of wire, that if those pieces of iron were taken away wire could be put where the iron had been, and so the same electromotive force could be secured with a Gramme armature with the same speed; but he omitted to say that by so doing he would double the resistance of the armature; hence Professor Ayrton should conclude from Professor Forbes' reasoning that the Pacinotti armature was superior to the Gramme. The protruding pieces of iron had a distinct advantage, but they had also a certain disadvantage, since they produced a want of symmetry in the armature, and so produced Foucault currents in the pole-pieces, causing heating and waste of power. But the whole question depended upon the result of experiment. If a Pacinotti armature were made sufficiently small

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—that was with the protruding pieces sufficiently far away from the pole-pieces, so that there was no serious heating—was there or was there not a greater or less electromotive force with the same amount of wire? He thought that if the size of the armature was diminished so as to prevent all serious heating, there would still be a larger electromotive force with the same amount of wire: in other words, that the Pacinotti armature was on the whole better than that of Gramme. A portion of Mr. Kapp's Paper was devoted to the consideration of what were the relative outputs of two dynamos of different sizes, a consideration of great practical importance in the days of large dynamos. As far as he knew, Mr. Deprez was the first to prove, or at any rate to draw attention to, the fact that the relative power of two similar systems of solenoids was as the fifth power of their linear dimensions: that if there were two dynamos exactly similar in shape, one a magnified image of the other, containing no iron, the output of the one would be as the fifth power of the linear dimensions compared with the output of the other. Of course, that required modifying if iron was introduced, also when account was taken of the necessity of having a sufficient surface for cooling. Professor Perry and he had come to the conclusion some years ago, that, when so modified, the fourth power of the linear dimensions expressed the relative powers of two dynamos; and he was, therefore, glad to learn that Mr. Kapp had practically arrived at a result with an exponent of 3½. The fourth power which he obtained some time ago had been still further diminished by Dr. Hopkinson as stated in his lecture, before the members of this Institution, on "Some Points in Electric Lighting," in which he had come to the conclusion that the relative outputs of two similar dynamos was about as the cube of their linear dimensions, or roughly speaking, as their weight.[1] In that investigation he thought that Dr. Hopkinson had not attached sufficient importance to the fact, that in making two dynamos of different sizes, one ought not to be made exactly the magnified image of the other; in fact, he had not taken into account what Mr. Kapp had considered, and what had previously been taken into account when Professor Ayrton arrived at the fourth power, namely, that there could be a relatively less clearance with a larger dynamo than with a smaller, and he thought that the exponent given by Mr. Kapp ought to be rather higher instead of lower than 3½. He agreed with most of Mr. Kapp's

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[1] "The Practical Applications of Electricity," Inst. C.E. Lectures. Session 1882-83, p. 100.

Prof. Ayrton. calculations, except where he stated (p. 139), "To put both machines into equal conditions as regards strains from centrifugal force, the speeds are assumed to be inversely as the linear dimensions." Surely, it ought to be the squares of the speeds should be inversely as the linear dimensions; that was the same thing as saying that the speed should be inversely proportional to the square root of the linear dimensions. It might be objected to this that the mass of the wire wound on the armature would be increased as the cube of the linear dimensions, while the sectional area of the binding-wire, used to hold the layers of conducting-wire on to the armature, would only be increased as the square of the linear dimensions. But there was no reason why only the same number of turns of binding-wire should be used with the larger machine as with a smaller; or why the distance between the sets of binding-wires should be increased as the linear dimensions of the armature. If the armature of the larger machine were rotated with an angular velocity, equal to the angular velocity of the smaller, into the square root of the ratio of the linear dimensions, the centrifugal force per unit mass would be the same; the total outward pull on the binding-wires would be increased, therefore, as the cube of the ratio of the linear dimensions; but this could be easily compensated for by an increase in the number of the binding-wires, and if the distance between the sets of binding-wires were increased, not in the ratio of the linear dimensions of the two machines, but as the square root of the dimensions, it was easy to show that the bulging of the conducting-wires in the larger armature would be exactly the same as the bulging of the conducting-wires in the smaller, due to centrifugal force. There was no difficulty, Professor Ayrton thought, in giving to the armatures of dynamos angular velocities inversely proportional to the square roots of their linear dimensions. Taking that into account, and using the numbers that Mr. Kapp had obtained, the result arrived at was undoubtedly $3\frac{1}{2}$; but he had stated, and he thought rightly, that the current might be increased in the proportion of 1 to q^2, where q was the ratio of the linear dimensions. Now, if the current was increased more than the linear dimensions, it was well known that the intensity of field would be greater. Taking, for example, q equal to 3, that was if the linear dimensions of one machine were three times as great as those of the other, the armature of the larger machine being rather more than three times the diameter of the smaller, on account of the diminished relative clearance, then the permissible current in the armature of the larger machine would be nine times as great; and, therefore, the intensity of the field would

Prof. Ayrton. be greater. It was difficult to say how much greater, since probably in the first instance the iron would be nearly saturated; but it was certainly greater. If it was assumed, that making the current nine times as great increased the field from 1 to 1·2, then the result would follow that the output was proportional to $1{\cdot}2 \times 3^{-\frac{1}{2}} \times 3^2 \times 3^2$ or $3^{3{\cdot}7}$ about. It might appear trivial to haggle as to the exponent 3, or 3·5, or 3·7; but it should be remembered that there was an enormous difference in the result. 3^3 for example was 27; $3^{3{\cdot}5}$ was 47; and $3^{3{\cdot}7}$ was 58. If, therefore, Dr. Hopkinson was right, the output of a larger machine would be only twenty-seven times as great as the output of a smaller; if Mr. Kapp was right, it was forty-seven times as great, and if Professor Ayrton was right, it would be fifty-eight times as great.

Mr. Crompton. Mr. R. E. Crompton stated that he was not a maker, but a considerable user of steam-engines for driving electric-light machinery, and he was therefore able to criticise to a certain extent the respective merits of the various high-speed steam-engines that had been mentioned. Electric-light engineers were rather severe critics on steam-engines, as they required exceptionally good ones. They needed first of all very great economy, and next great regularity of governing, which was not so easy a matter as had been commonly imagined. Governors, supposed to be perfect for driving cotton-mills, were found faulty when set to drive electric-light machinery. They also required another kind of regularity of speed, namely, regularity throughout each revolution of the crank-shaft,—what might be called equal turning—a point in which many engines were deficient; and they required, above all things, high speed. Mr. Imray had pointed out the difficulties to be encountered in high-speed engines, from the shock and noise due to the rapid reciprocations; he had also pointed out that the inertia of certain parts might be utilized to produce equal turning, a matter that was not so easily dealt with by proportioning the weights of the parts, as Mr. Imray had said it might be. Other speakers had also pointed out that a good deal might be done by cushioning. He did not think that Mr. Willans had dwelt sufficiently on the importance of the system of air-cushioning, introduced by himself. Mr. Crompton had used a good many of those air-cushioned engines, and he thought their employment was the most notable improvement that had been recently introduced in the construction of high-speed engines for driving electric-light machinery. The work done by these engines was of an extremely variable nature in two senses of the word. The same engine and the same set of machinery were in one case used with high-pressure steam exhausting into the

Mr. Crompton. atmosphere, and in another case, where there was low-pressure steam, exhausting into a condenser. Under those circumstances the task set before the engine-designer, to overcome shocks and vibration by steam-cushioning, was a very difficult one; Mr. Willans had said it was almost an impossible one, but he had met the difficulty with his air-cushioning in a remarkable manner. Mr. Crompton could personally testify to the success of Mr. Willans' system in comparison with the quick-running double-acting engine, the use of which Mr. Kapp now advocated. Some years ago he had the management of a number of engines of the latter class. These engines were built for the Indian Government, and ran at from 300 to 400 revolutions per minute; they were a constant source of trouble and anxiety, although they were just of that type which Mr. Kapp called a good "substantial" engine. At the above-mentioned speeds it was impossible to observe by sight or by touch the condition of the big end of the connecting-rods; and in some cases, after a long heavy run in dusty weather, the brasses of these big ends would be $\frac{3}{8}$-inch open. This was caused partly by wear, but mainly by the excessive knocking distorting the brass after it had once got a little open. For short runs no doubt these double-acting engines might be made to answer, but for long continuous hours of work, without possibility of stopping to adjust brasses, he advised electric-light engineers to avoid them. He had also another reason for fighting shy of them. If high economy was wanted, it was necessary to cut off early in the stroke, and this produced what had been called a sort of steam-hammer action. With high-pressure steam they cut off at a fifth or sixth of the stroke, so that the armature, or the fly-wheel added to it, had to carry the engine round the rest of the half revolution. It had been hitherto too much the custom to rely on the armature of the dynamo as a fly-wheel of the engine. But the armature of the dynamo could not bear such treatment. It was not a cast-iron fly-wheel all of one material, or at all events fitted metal to metal, but it consisted of a spindle of steel, of spokes of non-magnetizable material, of a soft iron core, and outside that of winding consisting of copper wire covered with insulating material. He thought it was right to call all insulating material hitherto invented "non-mechanical." It was not a material on which reliance could be placed to transmit tensile, transverse, or shearing-strains; so that it was necessary to protect the winding, whenever covered with insulating material, from all violent strains, particularly such as were liable to cause abrasive action. Such action as that with a high-speed double-acting engine, which obtained economy by an early cut-off, was most

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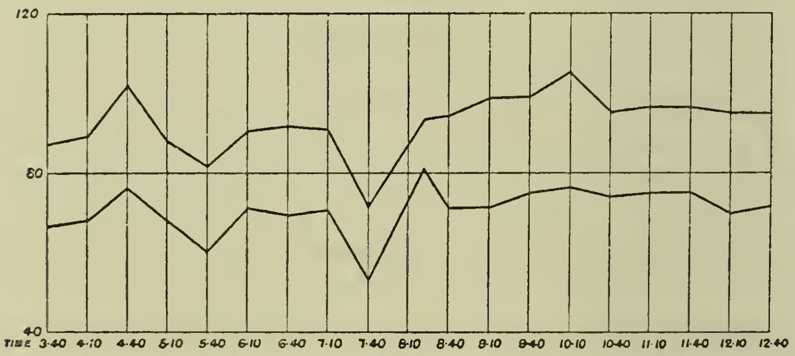
prejudicial to the armature, on account of the unequal speed throughout each revolution, first causing the core to get in advance of its winding, afterwards the momentum of the winding overtaking the core, and so sliding slightly forward, and producing abrasive action. That action always took place with early cut-off simple-engines, even when running at 300 revolutions per minute, and it was only a question of how long it was continued before the insulation of the armature broke down. Single-acting engines, he spoke particularly of those of Mr. Willans, which he had been chiefly using, were singularly free from that fault. He had succeeded, by his air-cushioning, and by the proportion of the weights of the parts, in getting over the difficulty in a great measure, so that down to moderate speeds it was not noticeable. A great deal had been said as to closed-up engines. The reason why he himself preferred them was the great facility of lubrication which they afforded, and the fact that it was possible to walk round such an engine without being splashed with oil. No open engine that ran 300 or 400 revolutions could on that account be approached within 5 or 6 feet. This difficulty did not exist in the case of a closed-up engine. The oil was put into the sight-feed lubricator at the top, and the speed at which it went into the engine could be observed. In the arrangements that he had carried out with dynamos combined with high-speed engines there were only two places for lubrication, namely, one at this sight-feed lubricator above the engine, and one for the single dynamo-bearing. Long experience had shown that these two advantages inherent to the single-acting closed-up engine, namely, the small attention required by the brasses and by the lubrication, enabled the cost of attendance to be greatly reduced below what was possible when double-acting engines, even if not very high-speed ones, were used. With single-acting engines he also preferred that the thrust should always be in one direction. It was easy to adjust one bearing of the dynamo to compensate for the wear that would take place in the bearings of the engines, provided the thrust were always in the downward direction: but if the cylinders were arranged radially, it was easy to understand that the wear did not take place in one direction, but in a triangle—that there was a tendency to wear the bearing into a three-cornered hole; consequently as the dynamo-bearing at the end distant from the engine did not become worn, whereas the other end of the armature revolved in a triangular path, the coupling must be a flexible one, otherwise a breakdown would speedily occur. By always having the bearing-thrust downwards there was no necessity

Mr. Crompton. for any flexible coupling. He bolted up the couplings metal to metal, and he had never had a bolt broken. During the present year he had fitted forty-six sets on that method, and there had been no hitch or breakdown in any one of the engines or dynamos. Next, looking at the engines and dynamos as combined, economy in fuel compared with useful electrical output was of the utmost importance. It was unwise to get an engine that would use a very little coal per indicated HP. if only a small proportion of that indicated HP. came out of the dynamo as electrical useful HP. Last year he had carried out a large installation, at Victoria Station. He had made careful experiments in comparing the indicated HP. in the steam-cylinder with the electrical HP. at the terminals of the dynamo, and after altering the ratios of speed several times, so as to get the best possible result, he found that when the engine was fully loaded, 166 indicated HP. in the engine gave 106 electrical HP. at the terminals of the dynamo, so that the electrical HP. was only 65 per cent. of the indicated HP., a large percentage of the loss being engine-friction, loss in shafting, belting, &c. When three-quarters loaded it was only 60 per cent., and when half loaded only 53 per cent. Since that time he had built three sets of large machinery of almost equal power, in which he had used Mr. Willans' engines, and he had made two long tests with them for the purposes of the present discussion. The first test was carried out a fortnight ago, and the other very recently. In Fig. 32 would be seen the result of a ten-hours' run with one of the sets. He was proud of that run, and he thought Mr. Willans ought also to be proud of it. The net result was 78 per cent. of the indicated

Fig. 32.



Ratio of useful Electric HP. to I.HP. (*i.e.*, total commercial efficiency) of a Crompton Dynamo driven direct by a Willans Engine.

November 28–29, 1885. $\frac{72}{600}$. No. 1,123 (Vienna).
Mean I.HP. 92. Mean E.HP. 72. Efficiency 78 per cent.

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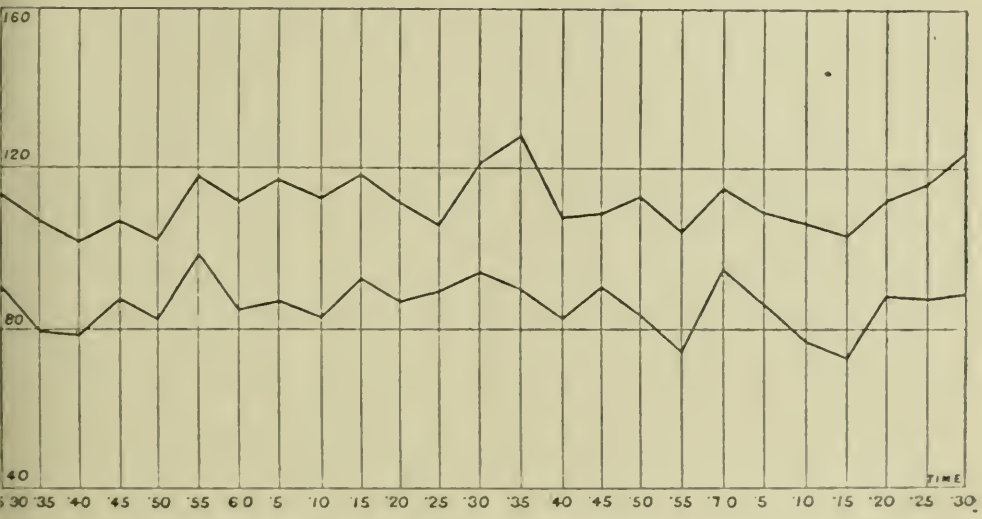
HP. obtained as electrical useful HP. at the terminals of the dynamo. Engineers would be satisfied with the weight of the water and the weight of the coal used during this trial. Mr. Willans was not satisfied with the coal-consumption, but he was. The consumption was 3½ lbs. of coal per net electrical HP. Fig. 33 represented the result of a similar trial, in which the economy was even greater than in that of the previous run, being at the rate of 31·5 lbs. of water used per electrical HP. per hour. The coal consumed was not recorded during this latter trial, but was calculated at the rate of evaporation of the former trial. It was equal to 3·2 lbs. per electrical HP. per hour. This was a result, he believed, that had never been obtained before in England. He might be permitted

Fig. 33.



December 12, 1885. $\frac{72}{600}$. No. 1,123 (Vienna).
Mean I.HP. 113·28. Mean E.HP. 86·8. Efficiency 77 per cent.

to compare it with two results that had been fully described in the engineering journals. Edison, at Ann Arbor, had obtained 68 per cent. of the indicated HP. as electrical HP., using an engine running at 210 revolutions, driving the dynamo by a belt, but he burned 17·6 lbs. of coal per electrical HP.; Van de Poele got 78 per cent., and burned 9·56 lbs. of coal per electrical HP. No doubt the American coal was not so good as English, but still there was a wide margin between the 9·56 lbs. burned in America and the 3·2 lbs. burned at Chelmsford. Mr. Kapp had written a Paper which would mark an epoch in the literature of the subject, and if he ventured to criticise one or two minor points he hoped that Mr. Kapp would not take it amiss. Mr. Kapp had promulgated

Mr. Crompton. the view that the drum-armature was far superior to the Gramme armature, and there he ventured to cross swords with him. He understood that Mr. Siemens had taken the side of the drum-armature to such an extent as to imply that there was no room for further invention. Mr. Kapp's were calculated figures, and were very interesting as such, but he did not think that they were based on experiment or practice. In fact, they were contrary to practice. He would at once admit that the drum-armature was theoretically superior in the number of inches of wire used in order to obtain a volt of electromotive force. But there were enormous drawbacks to its employment. Considering the necessity of having large radiating surface, and that the capacity of the output of a machine was in proportion to its radiating surface, it would be seen that, from this point of view, the drum-armature was as bad as the cylinder was good, and that everything gained in one direction was lost in the other. Mr. Kapp had put at the head of his list two machines, one of Dr. Hopkinson's and one of Mr. Crompton's. Nothing so much delighted him as to come into competition with Dr. Hopkinson, who had done more for this branch of electrical science than any one else, and if he could beat his master it would make him very proud. In this case Dr. Hopkinson had beaten him, in so far that he only required, according to Mr. Kapp's showing, 18·2 inches of armature-conductor to generate a volt of electromotive force, whereas he required 26·2 inches. If, however, the comparison of armature-conductor was made to show the respective percentages of loss in the armature, it would be found that whereas Dr. Hopkinson's loss was $2\frac{1}{2}$ per cent., his was 3 per cent., or only $\frac{1}{2}$ per cent. inferior. But was this worth claiming in the face of the fact that Dr. Hopkinson's was inferior in radiating surface to his own, almost inversely in proportion to the superiority claimed by Mr. Kapp, viz., as 26 to 18.

The slight advantages in the drum-armature might thus be challenged. But what were the disadvantages? The drum-armature had this peculiarity: the first turn put on was covered by every subsequent turn. When a drum-armature broke down it was always at the turn first put on. This first turn lay closer to the end of the core than any other and got the warmest, and the insulation failed at this point. He believed that Mr. Siemens would not be able to deny that when a Siemens armature came in for repair the inmost coil was the coil in fault, and every wire had to come off the armature. This was not the case with the Gramme ring, where the coils could all be drawn off independently, and it was only necessary to repair

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the one that was damaged. Professor Ayrton had spoken of the Pacinotti ring as being superior to the plain Gramme ring. Mr. Crompton had tested it carefully and had not been able to discover that superiority. One disadvantage of the Pacinotti teeth was that a large number of divisions could not be used on the commutator. Electricians conversant with the matter knew the importance of obtaining and maintaining a perfect surface on the commutator. The commutator was to the dynamo what the slide-valve and the face on which the slide-valve worked were to the steam-engine. As long as they were kept in good order everything went well, but directly the slide-valve face began to cut economy was at an end. It was the same with the dynamo; directly the commutator surface began to cut there was great loss in economy, the lights became unsteady, and the machine eventually would break down from this cause alone. The aim of a good designer of dynamos was to conquer that wear and tear of the commutator, and he thought that Mr. Kapp had not said half enough on the subject. All makers appeared to guard jealously the secret of how they obtained good results with commutators. He thought there need be no secret in the matter. What was wanted was to approximate the number of parts in the commutator as nearly as possible to the number of turns in the armature-winding—that was to get in as many as they could afford. Of course every additional part meant an additional slip of insulation, and an extra surface to be finished, which added considerably to the cost. Unquestionably Messrs. Siemens had been most successful in their efforts in this direction. Looking at any of their later machines, it would be seen that there were an enormous number of parts in the commutator. This could not be effected to a sufficient extent with the Pacinotti machine. It would be a matter of extreme difficulty to plough out a large number of small grooves in the sheet-iron disks forming the Pacinotti core. Mr. Kapp had stated that the single form of magnets was superior to the double form, requiring only 75 per cent. of the wire to get the same result. His calculations, however, had been entirely set at naught by the result of recent trials in America. The very best single machines that Mr. Edison could turn out were tried in competition with the best double machines that Mr. Weston could turn out. The Weston machines, he believed, laboured under the disadvantage of having cast-iron magnets. The result, however, showed that the commercial efficiency of the machines was equal, that of the Edison being 89·70, and of the Weston 89·92 per cent., so that practically they were

Mr. Crompton. identical. The HP. in the field (according to the calculation) was 1·80 per cent. of the whole in the Edison, and 1·70 per cent. in the Weston. Mr. Edison's weighed a great deal heavier than the other. Reducing the machines to equal output, the weight of the single-magnet machine was to the double-magnet one as 3,680 : 3,300. Mr. Kapp again, in order to suit the dynamos to his pet "substantial" engine, had cut down the peripheral speed to 1,400 or 1,500 feet per minute. He might say that this suited the engine, but not the dynamo. For if the dynamo ran double as fast it would give twice the output; or for a certain output it would be only half the cost per watt. A great deal might be said on the mechanical construction of dynamos. There was before the meeting a large array of different forms of field-magnets, but in his opinion there was only one good form, namely, the form that was easiest and cheapest to manufacture. Mr. Kapp had put cast-iron pole-pieces on the coned ends of his field-magnets. The question was, did he find that a cheap form of construction? Mr. Crompton's experience was that nothing was so difficult to get as an accurately-fitted coned joint. It was not a simple shape, and, although it looked well, he doubted whether it was cheap. He had exhibited one of the spindles and cores of his own machine. The great difficulty had been to make the dynamo a success from a mechanical point of view. Early designers, such as Gramme, had taken a ring-core made of iron wire, wound linen on to it as an insulator, and afterwards put on a great thickness of winding formed of the usual insulated copper wire. They had then glued a wooden hub into the centre, which was subsequently bored out and mounted on a spindle. It was needless to say that such a combination of wood, insulated wire, linen and other materials, all held together by friction and a little glue, did not stand well, or transmit properly the driving-strain from the spindle to the winding; the spindle or hub constantly worked loose. It was clear that the power must be transmitted direct from the spindle to the coils on which the work was put. He had endeavoured to do this, and yet to meet the electrical conditions by making a spindle with a large boss forged on it of best steel. In this he ploughed out grooves, and fitted into them by hand, as closely as possible, the phosphor-bronze radial bars that carried the core. The outer edges of these bars were dovetailed, and he slid the whole of the disks forming the core on to these dovetails. Since he had adopted this method not one core had come loose. The average pull on the periphery of the core was 500 lbs.; but the unequal turning of the engine, and the sudden startings and stoppings might frequently bring this up to $\frac{1}{2}$ ton. The insufficient care taken in early days, in providing for the transmission of the power from the spindle to the winding, was one of the causes which had given dynamos a bad name with mechanical engineers. Mr. Crompton.

Dr. J. HOPKINSON wished to say a few words, first, upon the method of calculating *à priori* the resistance of the magnetic field as set forth by Mr. Kapp. It so happened that rather more than two years ago he had begun the use of a method which was much of the same character, but which he carried further, so that he was able to make a fairly approximate calculation of the intensity of a field from a given configuration of iron, and a given winding. Professor Ayrton had also dealt with the same subject, and had come to the conclusion that Mr. Kapp was wrong in assuming that the magnetic resistance was proportional to the distance from iron to iron. Dr. Hopkinson thought that Mr. Kapp was right, provided the effect of the edges of the field, which was very material, could be neglected. Professor Ayrton had applied his conclusion to the relative advantages of the Pacinotti and Gramme armatures. He would not go into detail, but would only say that his impression was that the Pacinotti would be found the best arrangement for very small machines, and that the Gramme and the Siemens would be found best for very large machines. Where the line should be drawn between the two he was not prepared to state. He had applied the method of calculation, to which he had referred, to a comparison of one of his own improvements of Edison's machines with one of Edison's old machines, and the result was that in the case of his own machine, which had wrought-iron pole-pieces, the calculated magnetic resistance was greater than the actual, and in the Edison, which had cast-iron pole-pieces, it was less, not very greatly but materially. The reason why it so came out in the case of his own machine was, that the lines of force spread out from the horn of the pole-pieces of the armature, and gave an effective increase to the sectional area, and a consequent reduction of magnetic resistance. In the case of the Edison machine it was different. It had cast-iron pole-pieces, and there was a joint between them and the wrought-iron core of the magnet. Cast-iron became saturated much sooner than wrought-iron, and the limitation of the intensity of the field was due to the cast-iron becoming saturated at the limited area in which it was in contact with the wrought-iron. That led to another improvement in another machine, the Manchester dynamo, also constructed by Messrs. Mather and Platt. It would be seen that in that machine Dr. Hopkinson.

Dr. Hopkinson. the wrought-iron core was carefully taken into the cast-iron pole-piece, so that the sectional-area of the cast-iron, where it was in contact with the wrought-iron, was something like double the sectional-area of the core. About a year ago the same method of calculating the intensity of the field was applied in designing this Manchester dynamo. The configuration of the iron of the machine was first settled; then it was made a matter of *à priori* calculation what the winding of the magnets should be, and also what the effect to be expected from the machine would be. The calculations were made by Dr. Edward Hopkinson, to whom also the particular arrangement of the magnets was due, and the results came out very closely in accordance with them. So far as he knew, that was the first time that a dynamo of entirely new configuration had been calculated *à priori* with a result requiring no correction. It was an interesting illustration of how a proper analytical treatment of a subject of that kind helped in avoiding expensive experiments. About a year ago he made use of a similar method for the purpose of correcting some results of his experiments upon the magnetic properties of iron. Those experiments had been sent to the Royal Society, and would be published in its 'Transactions'; but as some time would elapse before their publication, and as many of the results were of practical interest in connection with dynamo-machines, he might be permitted to refer to them. Besides dealing with the magnetic properties of iron, he had made a considerable number of experiments upon the electric conductivity of various samples, for the purpose primarily of ascertaining whether there was any connection between the magnetic properties and the conductivity. It resulted in nothing in that direction; but one interesting result came out, that the electric resistance of the samples of cast-iron which he tried was ten times as great as the electric resistance of pure iron. That result had certain important applications to dynamo-machines. In many machines, such as the Brush, the Pacinotti, and some others, the armature was constructed with projecting portions of iron, which passed in proximity to the pole-pieces. Those projecting pieces tended to cause induction of currents in the pole-pieces, and, consequently, a waste of power and heating. That waste of power and heating would be directly proportional to the conductivity of the material, and consequently, if the pole-pieces were made of cast-iron there would be in such machines only a tenth part of the heating in the pole-pieces that there would be if they were made of wrought-iron. That result, he thought, was of some importance, and it went some way towards explaining how it was that the Brush machines, though they were heated a great

Dr. Hopkinson.

deal both in their pole-pieces and in the cast-iron cores of the armature, did not become heated to an absolutely destructive degree. Another point in his experiments was to ascertain the amount of loss of power which occurred on reversing the magnetization of a given quantity of iron. That was a point of considerable practical importance. It had been dealt with by Warburg and by Ewing. It was an important matter, because in nearly all dynamos iron moved in a magnetic field. Waste of power due to electric currents was avoided by subdividing the iron, but that did not prevent the loss of power due to the reversals of its magnetism. Applying his results to one of his own machines, the loss of power was found to be small in comparison with the loss of power due to electric resistance of the conductors in the armature, or in the magnets themselves. The question became much more important when the reversals of magnetization were more frequent; for instance, in the use of any form of secondary induction-apparatus it became a matter of importance to pay special attention to the loss of power and heating arising from reversal of magnetization. His Paper would contain results from a large number of samples showing their magnetic property, rendering it possible to determine the magnetic moment of certain simple shapes without further experiment. Another interesting feature was the extraordinary effect which the presence of manganese in steel had upon its magnetic properties. The presence of about 12 per cent. of manganese had the effect of making steel sensibly non-magnetic. The small amount of magnetism which remained would be fully accounted for by supposing that, mixed up with non-magnetic alloy of manganese and iron, there was a small quantity of free iron. Facts of that kind must ultimately have a very considerable bearing on any scientific theory, which might be propounded as to the nature of the magnetization of iron. Mr. Kapp had put forward a very just method of making a comparison of the relative excellence of dynamo-machines by comparing the intensities of their magnetic field; and as it was found that a machine brought out by Messrs. Mather and Platt and himself stood first on the list, he might be permitted to give a short account of it.[1] Figs. 34 and 35 represented sections of Edison and Edison-Hopkinson machines respectively, and Figs. 36 and 37 longitudinal sections of the armatures. It would be seen that the modifications of the old Edison machine lay not alone in the magnets, but also (which was quite as important) in introducing a larger quantity of iron into the armature.

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[1] See p. 151, and Plate I., Fig. 1.

Dr. Hopkinson. The armature was made up of thin disks of iron, and in the machine they had endeavoured to get in as many of those disks as possible. They left but a small space between the steel shaft and the disks, just enough to prevent any serious amount of magnetization. In addition to that they omitted the bolt-holes that were used in the Edison machine, and made the shaft itself a bolt. From a magnetic point of view, there had been practically no change in the machine since it was brought out two-years and-a-half ago. The most convenient way of comparing the field was to calculate the intensity of electro-magnetic induction in centimetre-gram-second units per square centimetre of the field. He would not stop to explain precisely what that meant, but it might be taken that the numbers which he gave were proportional to the intensity of the field, and they were given in those absolute units because they would be more useful for any purpose in making a direct calculation. At the time when the Edison-Hopkinson machine was introduced, the Siemens machine had no doubt a somewhat more intense field than the Gramme. The intensity of the Siemens field was a little short of 1,300 units per square centimetre, an Edison machine 2,200, and another Edison machine just short of 2,400. In the improved Edison-Hopkinson machine it was about 8,000, so that the improvement had the effect of producing *per saltum* a field between six and seven times as intense as the

FIG. 34.

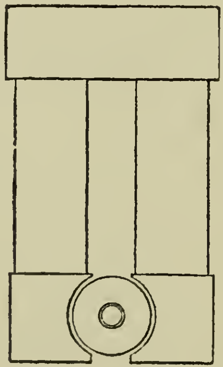


FIG. 35.

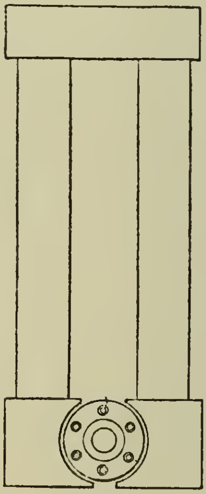


FIG 36.

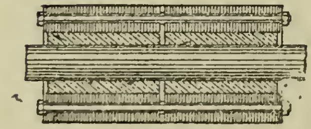
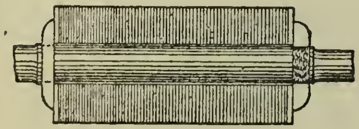


FIG. 37.



Dr. Hopkinson.

Siemens field, nearly four times as intense as the Edison field, and more than seven times as intense as the Gramme field. Since that time there had been a large development in the Gramme and other machines in the same direction. The intensities of the field in Gramme machines, such as were made by Mr. Crompton and Messrs. Mather and Platt, were as much in advance of the old Gramme machines of three years ago as the Edison-Hopkinson machine was in advance of the Siemens machine of that date. Mr. Edison had followed the improvements of the Edison-Hopkinson machine by shortening the magnets and increasing their sectional-area, with the result that the more recent Edison machines had fields of an intensity of nearly 4,000 per square centimetre, being nearly double of the older machines with long magnets, but still only half the Edison-Hopkinson. By also adopting the improvements of the armature, and either using wrought-iron pole-pieces or letting the wrought-iron into the cast-iron, Mr. Edison would further greatly improve his machine. For the sake of comparison it might be mentioned that the intensity of field in the Manchester dynamo was about 4,000. Mr. Kapp had spoken of the relative advantages of cast-iron and wrought-iron in the cores of the magnets. The advantage consisted in this: if wrought-iron was used the same field could be produced with a smaller core than was possible with cast-iron; shorter wire could therefore be used with less copper, and with a smaller waste of power in the wires. That, in his opinion, was a paramount advantage. Mr. Kapp had also dealt with the question of the permissible amount of heat in dynamo-machines per square inch of surface. That was a point which depended on a great many considerations. In regard to the machine itself it depended much upon the method of insulation, and upon the way in which the current of air got about. In his judgment the right method was to test any given type of machine by actually running it to something beyond what it would ever have to do. That was the proper course for any new machine, whether from the point of view of the seller, who did not want to sell a machine for more than it was capable of doing, or from the point of view of the purchaser. For that purpose the machine ought to be run for twelve hours continuously with some small percentage in excess of its nominal load. In the case of one of his own machines, the full load was put upon it, and it was run for a fortnight without stopping, and when it was examined it was found to be none the worse, so that the conclusion was that it had not been overloaded, and that it would be pretty safe to put it into any one's hands, even if it were occasionally loaded a little more than it was guaranteed to

Dr. Hopkinson. support. Mr. Kapp had also dealt with the relative advantages of multiple-pole dynamos and of dynamos with a single pair of poles, and he had come to the conclusion that there was no advantage in multiple poles. Dr. Hopkinson believed that the true answer to the question depended upon the construction of the armature. In the case of an armature in which the iron was shallow in comparison with its diameter, there could be no doubt that there was a great advantage in having multiple poles, because the core of the armature limited the total number of lines of force which could pass through it. There was, therefore, little use in having the pole-pieces of extended area in comparison with the sectional-area of the iron. In that case there was undoubtedly an advantage in using multiple poles. But, for his own part, he was disposed to choose a very different remedy. Mr. Kapp referred to the critical exciting power of the machine, and its critical resistance. In 1879 and 1880 Dr. Hopkinson had brought two Papers before the Institution of Mechanical Engineers,[1] in which he introduced a method of treating dynamo-machines by means of certain curves, which had since been called the characteristic curves of dynamos. In the second Paper, although he did not then investigate the question of the intensity of the field, he had dealt with the critical current for any given dynamo, the current at which it began to excite, and it so happened that he had used the term "critical" in connection with it. Something had been said as to the comparative advantages of the drum or Siemens type of armature and the cylinder or Gramme type. He was himself convinced of the great advantages of the drum type. He did not found his opinion upon the length of the wire and the consequent economy, because in well-constructed machines of either type the difference was comparatively small, especially in the case of large machines, but upon the fact that in the Gramme type of armature the wires had to pass within the armature, and when a considerable current was passing they were subject to the inductive action from the current itself much more considerably than in the case of a drum-armature. For that reason it would always be found that in the cylinder type of armature there was a greater disturbance of lead of brushes in similar circumstances than in the drum-armature, and that in large machines was a matter of great importance. Professor Ayrton had alluded to the lecture which he had delivered, and to what he had stated with regard to the effects of the comparative size of dynamos. He had then been dealing with that question from a somewhat abstract

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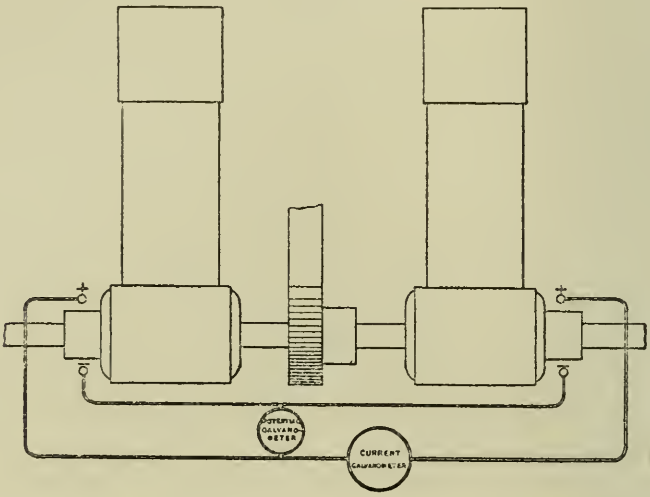
[1] Institution of Mechanical Engineers, Proceedings. 1879, p. 238; 1880, p. 266.

Dr. Hopkinson.

point of view, putting aside the practical question of the clearance required in different sizes of machine, his object being to call attention indirectly to the fact that the formulas which were then being given were misleading, and he had pointed out that taking certain considerations into account, the result would be about the cube of linear dimensions. Mr. Kapp had very properly laid considerable stress upon the commercial efficiency of dynamo-machines, and it was a matter of astonishment to Dr. Hopkinson that so little attention had been generally paid to that point. For the sake of a very small advantage in price many persons would put up with a considerable waste of power. It was still more astonishing that manufacturers of engines, when they had occasion to buy dynamo-machines to be driven by one of their own engines, laid so little stress upon that point; because if they bought a dynamo-machine of very high efficiency they could put in a smaller engine for it. In testing a dynamo-machine, the practice had been to measure the power expended in driving the machine and the power delivered from its terminals. At present manufacturers had got a pretty high efficiency, and they spoke, and spoke truly too, of obtaining 90 per cent. of the power applied delivered from the terminals as electricity. It was possible that in making such determinations there might be an error, say, of 1 per cent. Suppose, for example, that instead of 100 applied 99 was estimated, and instead of 90 delivered 91 was estimated. In that case efficiency was estimated as 92 per cent., the truth being 90. That difficulty fortunately could be completely avoided. The dynamo-machine was a perfectly reversible machine. It could be used not only to convert mechanical power into electricity, but to convert electricity back into mechanical power. Consequently, matters could be so arranged that it was only necessary to measure mechanically the power which was wasted. That could be done thus: let two machines be connected together mechanically as shown in Fig. 38. Let them be also electrically coupled together, and the electrical power be transmitted from the first to the second machine. Let there be a pulley on the shaft between the two machines, driven by a band, and the power transmitted by the band be measured. Supposing 100 to be applied mechanically to the first machine, a power of 90 might be delivered by that machine to the second machine. Of that 90, 80 would in the second machine be converted into mechanical power, which mechanical power would be transmitted by the shaft to drive the first machine. There would remain then to be supplied only 20 for driving the whole combination. The quantities to be measured were then 20 and 90.

Dr. Hopkinson. Suppose that instead of measuring quite accurately the measurements were 91 and 19·8. That 19·8 waste was divided between two, 9·9 for each. Consequently the first machine, apparently with an expenditure of 100·9, gave 91, so that although errors were made of 1 per cent. in each determination, in the final result of efficiency there was only an error of 0·2 per cent. Before concluding, he could not refrain from expressing his thanks to Mr. Kapp for his excellent Paper. It had dealt with the matter from a scientific as well as from a practical point of view, and it would long be regarded as a substantial contribution to the knowledge of the subject.

Fig. 38.



Mr. Walker. Mr. Benjamin Walker remarked that Mr. Imray had suggested the well-known notion that the heavy piston might be utilized in the ordinary steam-engine. He did not know whether Mr. Imray had given that as his own individual opinion, or simply as that of an ordinary engineer. Mr. Walker supposed that it was merely given as a universal opinion. He had paid great attention to the question of the heavy and the light piston. Eight or ten years ago, he made some very large steam-engines with cylinders 60 inches in diameter, with pistons of wrought-iron 3 inches thick, running at a speed of 1,500 feet per minute. The same class of engines was fitted with pistons 12 inches thick, of cast-iron practically solid, and running at the same speed. The results of the working were undoubtedly in favour of the light piston. It was obvious that the heavy piston could not be started without an undue expenditure of steam, and there must be some waste, however exactly the calculation might have been made, and the proportions worked out. There must be also loss from friction, so

Mr. Walker.

that all the power put into the piston at the beginning was not transmitted to the crank-shaft. To test the question more fully, he had constructed a steam-engine with a cylinder 18 inches in diameter and 2½ inches length of stroke, and another of the ordinary proportions, and length of stroke rather less than the full proportion. The stroke was one-and-a-half time the diameter of the cylinder, and the capacity of the two cylinders was the same. The two engines were put to do the same work, one without a belt, and the other with a belt. It required no diagrams to show which was the best. The engine with the heavy piston and short stroke was inferior to the one with the long stroke. There was, as it were, an unwritten law in mechanical engineering. If he were asked his opinion on the subject, he should reply that for a reciprocating-engine to go fast and smoothly and easily, the reciprocating-parts should be made as light as possible consistently with friction and with strength. For some years Messrs. Tannett Walker and Co., invariably made large pistons, sometimes 80 inches in diameter. The body of the piston was cone-shaped. That shape first eased the contraction, and next obviated the necessity for ribs which were very detrimental. The necessary strength was thus obtained with the least amount of weight. Mr. Crompton had spoken of the brasses having ⅜-inch play. He knew of 1,000 HP. engines, and he had examined them towards the end of a week's work, and could not see that the play was of the thickness of a bit of blotting paper. He had known them run for weeks without any play. If a reciprocating-engine was made to go at an unreasonable speed, and was not properly constructed, ⅜-inch play might be expected; but a marine-engine might be made to run five or six weeks without any trouble. In the case of one of the engines made by Messrs. Tannett Walker and Co., with the piston of wrought-iron, some dissatisfaction was expressed by Mr. Fox of the Leeds Forge, that the cylinder was not so clean as he expected it to be; that it was not bright and silvery. Up to that time the lubricant used had been tallow or melted suet, but they then applied vaseline, and the result was marvellous; the surface in a few weeks became as bright as silver, and the engine worked a great deal better. Mr. Imray had referred to the blower type of engine. Many years ago, he tried some very careful experiments with a blower of enormous capacity, and he found that nearly one-half of the cubical sweep of the blower was wasted through the joints. The reciprocating blowing engine might be made of half the capacity of the ordinary blower and do the same work.

Mr. Thornycroft.

Mr. J. I. THORNYCROFT rose to defend double-acting engines. It appeared to be the opinion of electrical engineers that double-acting engines could not be run at high speed for any considerable time. He had certainly been struck by the amount of wear in an engine after a day's run; the amount described was not, he considered, the common result obtained by a double-acting engine at the moderate speed mentioned. Messrs. Thornycroft and Co. were using the double-acting marine-engine to drive a part of the works. It was started about two years ago, and it ran at about 200 revolutions per minute, so that it was not a fast engine. There was an engine of Mr. Parsons' running at 12,000 revolutions, but it was not an engine in the ordinary sense of the word. Messrs. Thornycroft's engine had been in operation for two years, and during the last nine months it had been running practically night and day, being only stopped at meal-times, and not always then. The shaft was of steel. Mr. Allen had remarked that it was of importance to have the journals round. He thought that the journals must be pretty round, for the caps had not been off the shafts for two years. The main-bearings of the engine had needed no adjustment. They were originally screwed down, so that the upper and lower brasses were in firm contact, and now after two years were in a very satisfactory state. With regard to the other parts of the engine, the result was not so good. The small end of the connecting-rod required adjustment about once a month, and the eccentric straps, which were brass and iron, required taking up a little every week. The internal parts of the engine had given no trouble. They had used double-acting engines for driving fans, and had made a design for a fan-engine more than ten years ago, and had not since altered it. It was prepared for the first boat in the country that used air in the stokehole under pressure, and it was still in use. The engine performed well up to 1,000 revolutions per minute. The peculiarity of the engine was that the bearing-surfaces were very large and the working parts were light. Mr. Imray had shown the effect of a short connecting-rod to equalize the area of the diagram. In a single-acting engine of course the line representing the reaction—what would be a straight line in a long connecting-rod—became a curve, and was convex—the right way to increase the depth of the centre of the diagram, where the crank was able to do useful work. But the heavy parts of an engine increased the force on the journals at a time when the piston was near the end of the stroke, and this tended greatly to increase the friction. If the effect of a short

Mr. Thornycroft.

connecting-rod with a double-acting engine was considered, it would be seen that the acceleration-line was convex the wrong way when the pressure was on the side of the piston next to the crank-shaft. It took the bulk out of the centre of the diagram, and put it on the two ends. He was not in favour of a very short connecting-rod, although theoretically it might be an advantage in single-acting engines. The engine used in Messrs. Thornycroft's workshop was a compound-engine. They had no condenser, as they had no opportunity of applying water conveniently. They used a compound-engine working above the atmosphere. He recently had some diagrams taken, and it was found that they were doing nearly all the work in the high-pressure cylinder, and about 3 HP. in the low-pressure cylinder. He suspected the cause was that the jacket was not in operation. He was surprised to find how great an effect the jacket appeared to have, on turning the steam into the jacket. The engine went much faster, and the steam had to be reduced to keep it under restraint. The indicator which he had on the low-pressure engine immediately began to show increased change of pressure within the cylinder. It was found that the effect of putting on the jacket was to increase the work by about 15 HP. in the low-pressure cylinder. With regard to Mr. Willans' engine, he was glad to hear that the electrical HP. was so good, but considering that high-speed diagrams were liable to under-estimate the power of an engine, and the high duty got out of dynamos, it would seem that there was still room for improvement. In regard to the amount of coal consumed, Mr. Willans might be congratulated on the result. He liked the symmetrical arrangement of Mr. Willans' engine, and he admired the idea of an engine in which apparently everything was turned up in the lathe. This gave great facility for adjustment, with little trouble. But from the experience of the cards taken from his own shop engine, he should be disposed to recommend Mr. Willans to jacket his cylinder. Mr. Willans had stated that in order to equalize the momentum, he used a cushion of air. That might be an advantage on the whole, but he believed the cushion would consume a good deal of the work. When the air was suddenly compressed it would become heated and give out part of the heat to the cylinder, and when that air again expanded he was afraid that all the work would not be got out of it that was put into it. Mr. Allen in his engine put the cranks opposite, and in that way had a considerable advantage in taking the work off the main-bearings and lessening the friction. When an engine was only started occasionally, of course cranks at right-angles

Mr. Thornycroft. were not required. The effect was partly sought to lessen vibration. In a yacht which they had built, the same object had been attained by putting three cylinders at equal angles, and the result was satisfactory. If the lightest possible engine was desired, it should be double-acting, for the weight of the cylinder-casting was not doubled by making the engine double-acting, nor even greatly increased, although this change doubled the power obtainable from a given capacity of cylinder. And when the framing was considered, it would appear that the compactness due to allowing the connecting-rod to enter the working cylinder might be purchased too dearly. Air and water thrown into the cylinder must cool it and cause great condensation of the steam. There was no doubt great convenience in a single-acting engine, where the stresses on the bearings did not change sign, but this property had the disadvantage of reducing the oil film to a less thickness than the film that might be used in a properly-adjusted double-acting engine, where the change of pressure served to admit space for oil as frequently as this change occurred. With a thick film of clean oil he thought no perceptible wear took place, and that blowing fans for a moderate blast were best driven direct by double-acting engines with one cylinder only.

Mr. Imray. Mr. Imray said it was not necessary that he should occupy much time in reply. His Paper, he thought, might have been more severely criticised than it had been, as it opened out questions which perhaps deserved a little more criticism than they had received, and some of his propositions might be considered a little startling at first. Very few of the speakers had said anything in opposition to his views except Mr. Tower and Mr. Fielding, and he hoped they would understand that when he spoke of rotary-engines, he did not (as he had particularly mentioned) include those which were of a reciprocating character. In their engines there was a constant reciprocation going on, and that reciprocation would be found, if he had had time to compute the formula, to have had the same effect in respect of inertia as in an ordinary reciprocating-engine. He had therefore excepted those from the conclusion he had drawn with regard to rotary-engines. That conclusion was based more upon engines like the reaction-wheel or a turbine, or those in which a direct current of fluid was the cause of the continuous rotary motion. He had not gone into the question of the rotative effort upon the fly-wheel, because he thought it was sufficient in opening a subject of that kind to look simply at the impulsive effort in the cylinder, for that impulsive effort in the cylinder was, after all, the measure of the strain upon the joint of the connecting-rod, and there he stopped. Mr. Walker had stated that engineers had a sort of natural instinct as to what was right and what was wrong. Mr. Walker might perhaps recollect that a great many years ago engineers had some queer notions about mechanics, which it required a good deal of theory to put right. He had offered in his Paper a little bit of theory, which he wished mechanics and engineers generally to look into somewhat more than they had hitherto done. He might be right or wrong, but he thought that the field was one worth studying. With regard to Mr. Thornycroft's statement, he had always had the highest opinion not only of double-acting engines generally, but especially of Mr. Thornycroft's engines; but the question was not as to the comparative merits of one sort of engine and another, but rather as to what sort of engine was best adapted for very high speeds. He was convinced that for very high speeds a single-acting engine, which had no alternation of strain in its moving parts, was the best. Mr. Imray.

Mr. Kapp.

Mr. Kapp, in reply, observed that as far as rotary-engines were concerned the discussion had been purely descriptive and did not need any remark. Regarding reciprocating-engines the discussion might be described as a battle of the single-acting against the double-acting principle, and although one speaker had upheld the former, it was doubtful whether the faith of the engineering world at large in double-acting engines has thereby been shaken. If the question of speed were disregarded for the moment it would be admitted that the majority of engines at present in use for general purposes were of the double-acting type.

In a large marine-engine, in a locomotive, or in a mill-engine, there existed, to a certain extent, all the difficulties of cushioning at variable loads and speeds, and the reversal of strains pointed out by Mr. Willans; yet these engines were made to work without knocking, vibration, or other inconvenience. In a properly-designed engine the reversal of strains took place gradually, and what little knock there might be, was deadened by a film of oil between the working surfaces. The difficulties were increased at higher speeds, but could even then be met in a satisfactory manner, as was proved by the Porter-Allen and other engines. Until very recently it was believed that dynamos could only be driven direct by some special type of rotary or other closed single-acting engine, and he had endeavoured to show that the double-acting engine, which an experience of many years had proved to be the best fitted for general purposes, could with greater advantage and certainty be used for this particular work. This

Mr. Kapp. had brought down on him the severe criticism of Mr. Willans, who had noticed many points, some directly, and some only remotely, connected with opinions expressed in his Paper. He would limit his reply to the former. The cost of single- and of double-acting engines might be taken as equal for the same capacity of cylinder, and to obtain equal power the speed of the single-acting engine must be double that of its rival. Now Mr. Willans fixed the speed for an engine having 12 inches length of stroke at 440 revolutions per minute, whilst the Porter engine of the same length of stroke ran at 350 revolutions. It had therefore an excess of power of 60 per cent. Mr. Willans objected to his using the term piston-speed as misleading, but he had given the length of stroke as well, a practice in common use with engineers. He was in favour of high speeds for dynamos, and suggested that his dynamos at the International Inventions Exhibition would have given more output, and a higher commercial efficiency, if run at 680 revolutions per minute instead of at 340. The same might be said of any other machine; for instance, of the Crompton dynamo, driven by a Willans engine, which did about the same work at 550 revolutions per minute. It would puzzle Mr. Willans to say why he did not apply his precept in his own case. His statement about the commercial efficiency was incorrect, and he seemed to have confounded the electrical and the commercial efficiency. The former would at a higher speed be increased, but the latter might actually become lowered, owing to greater waste due to magnetic friction, as had been pointed out in another part of the Paper, and had also been confirmed during the discussion. Mr. Mordey considered that he had ridden the low-speed hobby too hard, and that with an increased diameter of armature, necessitated by low speeds, the magnetic friction must be considerable. As regarded the first observation, it was not the maker, but the buyer of a dynamo who decided the speed, and the general tendency had been towards low speeds and would probably continue in that direction. With respect to the latter observation, he thought he had entered upon debatable ground, for if Mr. Mordey considered his 16-inches armature in an 18,000-watt machine too large, what could he say for an equivalent disk-armature, the diameter of which was necessarily much larger? But Mr. Kapp could not agree with his conclusions that a large armature caused a reduction of commercial efficiency. There was certainly a larger mass of iron to magnetize, but on the other hand the magnetic resistance of the whole machine was less, and the energy required for producing the field was considerably reduced.

Returning to the remarks by Mr. Willans, he had mentioned two ways by which knocking in double-acting engines could be avoided; one way by compression of the exhaust, the other by very heavy working parts. He did not know why it was argued that these methods were directly opposed to each other. He thought that a combination of both might sometimes be adopted. Mr. Willans had argued that at 700 revolutions per minute it would be impossible to balance the engine shown in Plate 2, Fig. 27, and take up the shock by heavy working parts. But he had no desire to run the engine at so high a speed, and if it were required to balance an engine of that size by the Porter method, he would reduce the diameter and increase the length of stroke. Mr. Willans also maintained that single-acting engines were more economical, and remarked that no figures had been submitted as to the economy of double-acting engines. When the Paper was written Mr. Kapp had no definite figures to give, but since then, tests had been made, and, as mentioned by Mr. Allen during the discussion, 77 per cent. of the indicated power had been converted into electrical energy in the external circuit. Mr. Willans had given 80 per cent. as the return in one of his engines driving a Crompton dynamo, about five times the size of that shown by Plate 2, Fig. 27. This was a better performance by 3 per cent.; but he had hoped with a plant of equal power to attain and perhaps exceed 80 per cent. There was evidently little difference between the two systems as far as economy was concerned. But for simplicity, he maintained, that the double-acting engine was preferable. An inspection of the Willans and the Armington-Sims engines would show that the latter type was less likely to give trouble, and was easier to repair. Mr. Alexander Siemens had directed attention to the fact that in the Table giving the length of armature-conductor per volt Mr. Crompton's armature required a length of 26·2 inches, whilst the Kapp armature required 35 inches. But the former was a long and the latter a short cylinder. If both were of the same type the difference would be reduced by about 4 inches. Mr. Siemens had also said that the Kapp machine contained 50 per cent. more copper than the Crompton; but in making this statement he had overlooked the fact that a dynamo-machine consisted of something else besides an armature. By using an excessive amount of copper on the field-magnets and lavishing exciting power on them, it was possible to produce a large number of watts from a small weight of copper on the armature; but it was not economical. There was always a great deal more copper on the magnets than on the armature to begin with, and a slight saving in the latter was very Mr. Kapp.

Mr. Kapp. dearly paid for in the former. This was so well understood by dynamo makers, that even the best cylinder-machines had seldom less than 1 yard per volt. The Crompton machine mentioned in the Table was one with an exceptionally strong field; the two machines which were in the International Inventions Exhibition had not quite so strong a field, and required about 33 inches per volt. He could not agree with Mr. Siemens that it was waste of energy to try to invent new forms of armature, since the best possible form had already been invented by a member of his firm years ago. If such a doctrine were accepted progress would cease. It was better to try and to fail, than not to try at all; and the mere copying what one great firm had done was repulsive to the feelings of professional men. His views coincided with those of Mr. Siemens in regard to the necessity of building dynamos in such a way that they could be easily repaired; and he would even lay greater stress on such designs as would avoid as much as possible the need for repairs. In both respects the Gramme armature had a great advantage over the Siemens armature. In the former, neighbouring coils were never at a very great difference of potential, and each coil could be renewed without disturbing the rest. In the latter, wires of very different potentials crossed each other at the ends of the drum, and to reach any particular coil it might be necessary to unwind a large portion of the wire.

He could not follow Mr. Bedford Elwell in his reasoning intended to prove that a thin wire could do more work than a thick one. The basis for his argument was that the magnetic induction between the pole-piece and the armature varied inversely as the square of the distance. This was certainly not the case. Mr. Trotter had shown an interesting method by which he arrived at the diameter of the armature. No doubt wire could be saved by it; but it must not be overlooked that by making the inner diameter too small, the difficulty of ventilation was increased, whilst at the same time the cooling-surface was diminished. The magnetic resistance of the air-space was thereby also increased, and more copper and more energy were required to excite the field-magnets. The remarks of Mr. Esson about the value of Pacinotti projections had been answered from a scientific point of view by Professor Forbes. It would seem as if there was not much difference between a toothed and a smooth core, as far as the creation of electromotive was concerned; but the toothed core had the mechanical advantage of positive driving. Professor Forbes protested most energetically against his introducing the letter *z*. But he was himself a little inconsistent, for after declaiming

Mr. Kapp. against the presumption of the Author writing his own formula in his own way, he used the letter N, introducing another new symbol.

Attention had been directed by Professor Ayrton to the similarity existing between electric and magnetic resistance. The curve showed that the magnetic resistance did not increase as the distance increased, but increased much more rapidly. In estimating the magnetic resistance, as he had done, mathematically, for the purpose of finding the number of lines passing across the field, he had to lay down one condition: that the air-space should be infinitely small in comparison with the extent of surface. Unless that was done the formula was not correct. When he said infinitely small, he of course meant very small. Mathematicians knew well that they could not deal with the infinite in actual designs, but they approached it, and in dynamos it was really approached. The distance between the polar-surface of the magnet and the surface of the core in a large dynamo might be $\frac{1}{2}$ inch; the extent of surface would be 20 inches one way, and perhaps 16 inches the other. Comparing such proportions, it might be safely assumed that magnetic resistance followed the mathematical law which was only correct if the distance was very small; or, as Dr. Hopkinson had pointed out, if the effect of the edges was neglected. Professor Ayrton had stated that what he had said about velocity was not correct. He had all through the Paper assumed the peripheral velocity to be constant for different diameters of armature, and therefore the angular velocity must be inversely proportional to the diameter. This would, as in the case of a flywheel, give an equal factor of safety with regard to the danger of the armature flying to pieces under the influence of centrifugal force. Professor Ayrton had said that the increase of power with the increase of linear dimensions should be more than $3\frac{1}{2}$, because the field would increase as the current would increase, and thus the electromotive force would also increase. That was quite true theoretically, but the field commercially did not increase, because it was already near saturation. Mr. Crompton had spoken of the double-acting engine. But if engines in India were worked very badly that was no reason why they should not be worked well in England. Mr. Allen's engines had been used on steamers, and had run for six weeks without stopping at speeds varying from 200 to 600 revolutions per minute. Mr. Crompton had been right in calling attention to the practical difficulties of the drum. In his Paper he had said that the drum was the best on theoretical grounds. But not so in regard to practical grounds; there was

Mr. Kapp. but little radiating surface, and no means of getting air to the core. Dr. Hopkinson had affirmed that work was expended in magnetizing the iron of the core, no matter how much it was subdivided. This work must reappear in the form of heat, and the drum-armature should therefore be found defective in this respect. Dr. Hopkinson said that it did not become heated, and Mr. Kapp could only assume that some element was present which prevented this heating, but which as yet was not understood. According to present knowledge, it ought to become heated. He was afraid that Mr. Crompton, in speaking of the merits of single and double field-magnets, had misunderstood him. The statement that he saved 25 per cent. was taken from the actual prime-cost books. He had made many machines of the same form, and always found them come out cheaper in the prime-cost book. In comparing the Edison and Weston tests, Mr. Crompton did not speak of the length of the wire, but of the power required to magnetize, which was a totally different thing. If plenty of copper was used, power could always be economized for magnetizing. Unless it was known what the amount of copper was in the Edison field, and what in the Weston field, there were no means of judging between the merits of the magnets in these two machines.

Correspondence.

Mr. Alley. Mr. Stephen Alley remarked that his experience of rotary-motors, or pulsating rotary-motors, had not been satisfactory. At one time he thought it was within the skill of a good mechanic to produce, by absolutely correct workmanship, a durable motor of this type; but, after many trials, he came to the conclusion that the same amount of ingenuity and care that had been devoted to rotary-motors could be much better utilized in producing a reciprocating-motor to do the same work. His great difficulty was the unequal wear of the revolving parts, due to the increase of speed the greater the distance from the centre of rotation. He also found that in motors of this type the consumption of steam was excessive. This in great measure was due to the large proportion borne by the steam-ports to the capacity of the cylinders, the consumption increasing very much after the moving parts had become worn, showing that on account of the unequal wear of the parts, the life of such motors must of necessity be a short one. He therefore thought that rotary or pulsating rotary-motors would not prove of much use, except in cases where

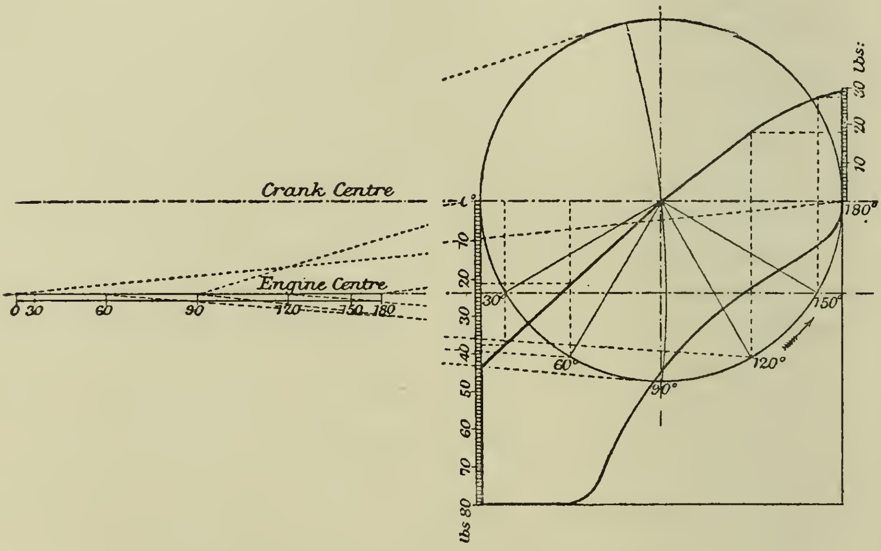
Mr. Alley.

the space was too limited to allow of the introduction of motors of the reciprocating form, and when economy in fuel was not a consideration. To get a very high power out of a small motor by increase of revolutions must necessarily be wasteful, even in reciprocating-motors, that were not carefully designed, as the steam used to fill the ports and the clearance at the cylinder ends did not give much useful effect. Whatever loss there was through this cause had to be multiplied by the revolutions of the motor. This loss, however, in small motors of the reciprocating type, should not generally increase with wear, as the parts were made to follow up and keep close to one another as wear took place; whereas in the case of rotating valves or their equivalent, such as he had seen in use and employed in rotary-motors, and in some reciprocating motors the loss of steam invariably increased as the parts became worn. One of the great advantages possessed by a reciprocating-motor over the rotary (outside the unequal wear of parts) was the ease with which the steam could be cut off and expanded within the cylinders. A decided preference had been given by Mr. Imray to single-acting high-speed engines, because of the work being entirely done by a push. This he considered to be correct, as the slightest slackness of joints in engines actuated alternately by a push and a pull, if running at a high speed, must give a jerking uneven motion when the joints became worn; and although these engines, such, for example, as Mr. Allen's, which were, no doubt, very beautifully fitted, might be perfect on leaving the workshop, in the case of engines that drove dynamo-machines particularly, they often did not receive the attention requisite in double-acting motors. Consequently joints were allowed in many cases to work slack, so that no matter what precautions the manufacturer might take to produce good workmanship, his efforts were too often counteracted by carelessness or inexperience on the part of the driver of the motor. In the case of single-acting engines which transmitted motion to the crank by means of a push, this uneven motion could not occur. When the connecting-rod bearings became worn they simply followed up the crank, and could not lift from it, if the piston on the up-stroke was cushioned by exhaust steam.

The "Westinghouse High-Speed Automatic Engine" was of the single-acting type, and it would be noticed from the diagram, Fig. 39, that the centre of the cylinder was not over the centre of the shaft, but half a crank to the left or go-ahead side of the shaft. This gave that type of engine the effect of having a connecting-rod double its actual length. He had made out an

Mr. Alley. inertia-diagram of the accelerating pressure (as set out by Mr. Imray) for a Westinghouse engine having cylinders 12 inches in diameter by 12 inches length of stroke, working with a steam-pressure of 80 lbs. per square inch cut-off at $\frac{3}{10}$ stroke, and he had worked out a steam-diagram to suit, which was combined with the inertia-diagram already referred to. The speed assumed was 300 revolutions per minute, or 5 per second. The length of connecting-rod was five cranks, but due to its relation to the centre of the cylinder, as previously explained; the greatest angularity of the connecting-rod, when being pushed forward, was only equal to that of a connecting-rod ten cranks in length. In calculating a curve-line he had taken the connecting-rod at ten cranks as the basis, and the line of curve on the steam side of the diagram came out nearly straight, and resembled diagram Fig. 1 of Mr. Imray's engines, with a connecting-rod of infinite length; but on the negative side of the centre line it assumed more of a curved form. The part included within thick lines on the diagram corresponded with the diagram Fig. 3, p. 117, but the engine ran in the opposite direction.

Fig. 39.



Westinghouse Automatic Engine.

Scale, ⅛.

Diameter of cylinders, 12 inches; length of stroke, 12 inches; length of connecting-rod, 30 inches; ratio to crank, actual 5; ditto, effective, 10; steam pressure, 80 lbs. per square inch; revolutions per second, 5; weight of piston, cross-head and connecting-rod, 295 lbs.; pressure per square inch on piston, due to weight of above, $\frac{295}{113} = 2 \cdot 6$ lbs.

It had been stated by Mr. Imray, p. 118, that a short connecting-rod was objectionable on account of the strain thrown on the slipper, but he seemed to think that it might be more than made up for in high-speed motors, by the great equalization of the effective pressure throughout the stroke, resulting from the parallelism of the inertia-curve to that drawn by the indicator, this parallelism being increased by the use of the short connecting-rod; but Mr. Alley considered that the parallelism of the inertia-curve to the indicator-curve was as good as, if not better than, any shown by Mr. Imray, and the diagrams given by Mr. Imray were based on a length of connecting-rod of only three cranks, so that with the "Westinghouse" method his objections under these circumstances to a long connecting-rod could be abandoned. Mr. Imray had also objected to engines with the cranks diametrically opposite, that they had only the fly-wheel to take them over the dead-points. This was no doubt the case in engines of the ordinary type of construction; but in the Westinghouse engine the crank-shaft being placed a half crank out of centre of the cylinder, to a great extent, if not entirely removed this objection; in point of fact these engines could be run without any fly-wheel, by a partial balancing of the crank. Mr. Alley.

He submitted some of the indicator cards, and results of indicated and effective brake HP. trials which had been taken with an engine of the dimensions above referred to. Fig. 40 was a full-power card cut off at $\frac{1}{2}$ stroke. Fig. 41 was the card which he recommended as the most economical for small high-speed engines cutting off at about $\frac{3}{10}$ stroke (it was well known amongst practical engineers that a higher grade of expansion in a single-cylinder engine was not economical). Fig. 42 was merely a friction-card, taken when the engine was run without any load. By reference to the data taken with the diagrams, it would be observed that the loss due to friction, or the difference between the indicated and effective HP., was very small. After the first trials it was thought there must have been some mistake in the tests; but repeated tests had given on an average the same results, and doubts disappeared when it was taken into consideration that only four different parts in the engine could cause friction, namely, the pistons, the connecting-rods, the crank-shaft bearings, and the piston slide-valve common to both cylinders. The thrust of the connecting-rod did not amount to more than $\frac{1}{3}$ lb. of mean pressure on the length of the stroke for every 100 lbs., pressing on the piston with a coefficient of friction of $\frac{1}{10}$, and as the piston slide-valve was balanced, the power required to drive it was inappreciable; the greatest amount of power absorbed by friction was therefore due to the bearings only. Of course, in the smaller class of engines more frequently used for

Mr. Alley.

Fig. 40.

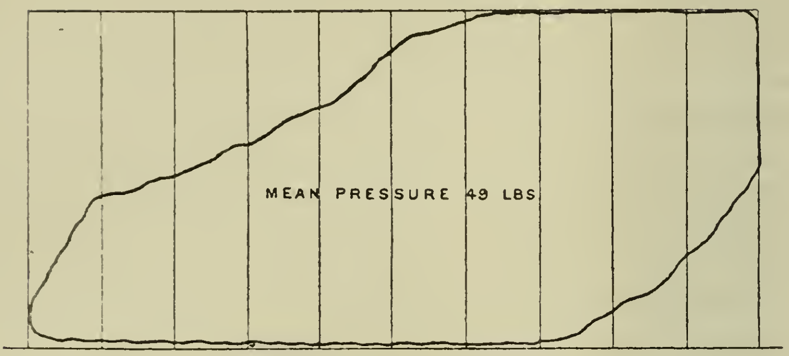


Fig. 41.

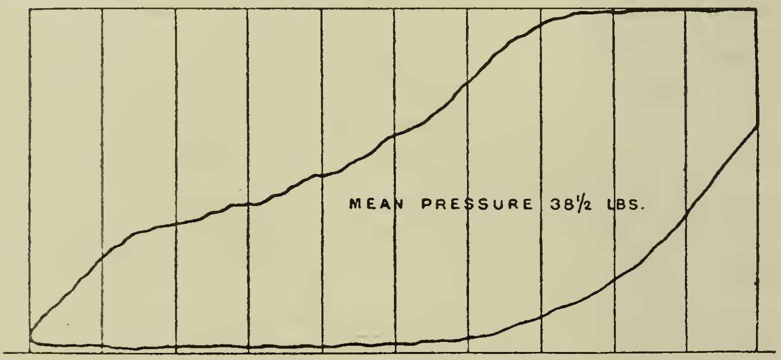
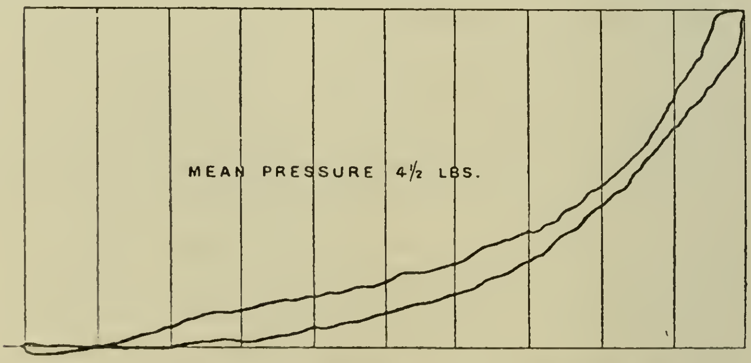


Fig. 42.



——	Fig. 40.	Fig. 41.	Fig. 42.
Number of revolutions	298	298	305
Regular speed, revolutions . . .	300	300	300
Loss of speed ,, . . .	2	2	..
Gain of speed ,, . . .	..	..	5
Percentage of variation	0·6̇	0·6̇	1·6̇
Indicated HP.	100·67	79·1	9·24
Effective HP. by brake	89·37	71·0	..
Loss of HP. by friction	11·3	8·1	..
Percentage of loss	11·25	10·25	..

Mr. Alley.

driving dynamo-machines, the loss must be greater than in this size, as the waste due to steam-capacity of ports, and clearance of pistons, could not be reduced in proportion to the size of cylinder.

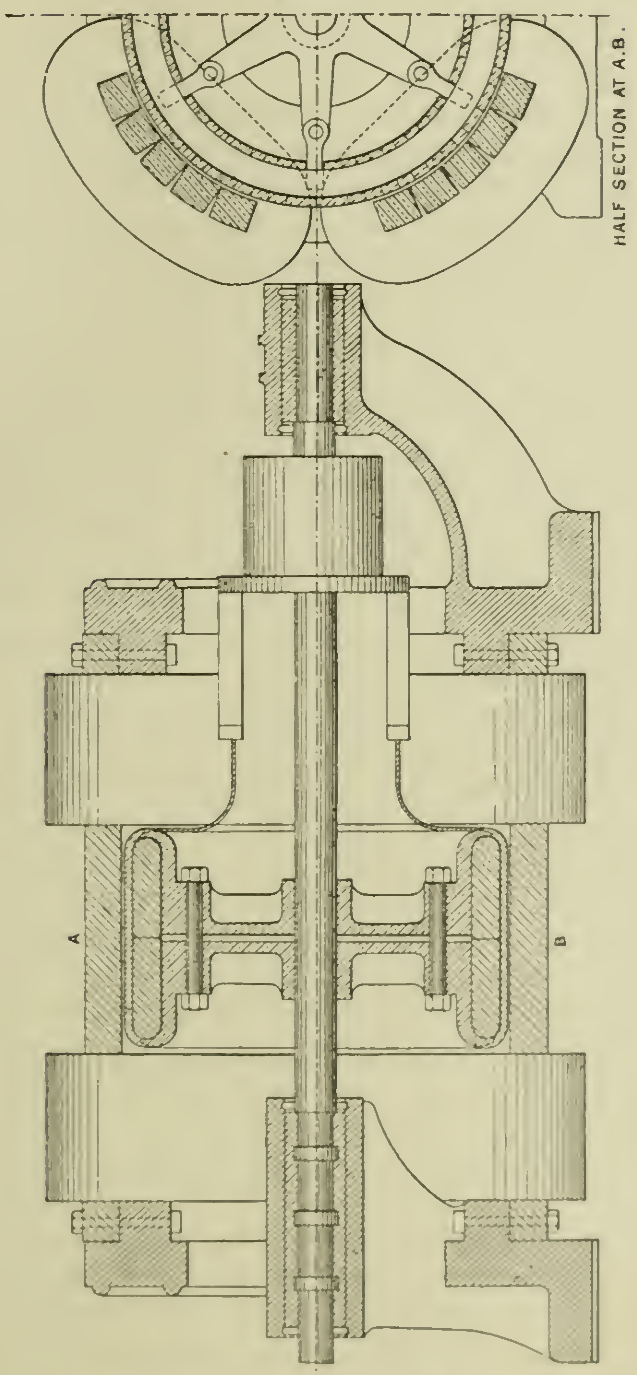
One difficulty with regard to motors driving dynamo machines was the unfavourable position in which they had often to work on board ship, particularly in the merchant service. There was often no suitable place for engines and dynamos; and consequently they were crammed into some awkward corner in the engine-room where it was almost impossible for the engine men to attend to them properly. In one case which came under his notice the engines were placed in what had been a small engine-room store-closet, close to the boilers. The heat in this place was never below 120° Fahrenheit, and the consequence was that it was almost impossible to give proper attention to the oiling of the bearings. Another difficulty on board ship was the ignorance of many sea-going engineers. They seemed to think that because the engine was driving the dynamo-machine it was not an engine, but part of the electric apparatus; consequently they feared to touch it, and as a result the engines were too often entirely neglected. No doubt difficulties of this nature would ultimately be overcome, but he believed that all makers of high-speed engines would admit that they had suffered from this cause. If electrical apparatus was placed on board ship, it surely was entitled to accommodation and ordinary attention like other machinery. It could not be too strongly impressed on naval architects and ship-owners, that it was necessary to provide suitable room and attention for electric installations on board ship, if they wished for success.

In connection with Mr. Imray's Paper on High-Speed Motors, it would be most interesting to find out the exact consumption of steam per brake HP. by practical tests, conducted by a committee of experts, in no way connected with, or interested in, the manufacture of high-speed motors. To carry out an investigation of this nature Messrs. Alley and MacLellan would be glad to supply a Westinghouse engine, and if necessary pay a portion of the cost of such experiments. He had no doubt that other makers of good high-speed motors would, for the sake of science, also be glad to join in the experiments to show what could be done by high-speed motors. If such data as the proposed committee would gather from these experiments were published, it would determine the vexed question of consumption of steam in high-speed motors of various types, in proportion to the actual available

Mr. Alley. power given out by the crank-shaft. He had no faith in power calculated from indicator diagrams in high-speed motors. These diagrams were only useful to show the points of cut-off and the expansion of the steam. A test by brake that would give the useful work after deducting the power absorbed by friction in the motor, and the loss of steam in the ports, &c., was wanted independent of statements made by inventors and manufacturers, which might be considered more favourable than they deserved to be. With reliable data of this nature, engineers would have more confidence in using high-speed single-acting motors.

Mr. Andrews. Mr. J. D. F. Andrews observed, with respect to multipolar machines, that Mr. Kapp had thrown doubt on the existence of any advantage attributable to such dynamos compared with two-pole machines, in the absence, he thought, of accurate knowledge of the dimensions of those of more recent design. He supposed a difficulty in obtaining a sufficient polar-surface; this, however, could not exist, seeing the diameter of the armature need not be limited. There was, however, no advantage in extending the surface of the magnetic pole beyond that which gave the armature core its highest magnetism, and there was no object in presenting more thickness of core to be magnetised than was required to produce the greatest effect in the copper wire surrounding it consistent with practical dimensions. The machine, shown in section in Figs. 43 was a four-pole dynamo, designed to produce the same results as a two-pole dynamo at very much fewer revolutions. The core of the armature was a cylinder or ring of iron formed by winding wire so firmly on a cast-iron mould, that when removed it was almost like a solid ring; this was carried by six grabs or spokes direct from the shaft, and a copper bar was wound on to it in fifty-nine sections, four convolutions each. The large diameter of the core admitted of a very thick bar being wound on, thus obviating the necessity of joints at any other point than with the commutator. The fifty-nine sections were connected in series, not, however, in succession, but diametrically across; these connections were a slight loss, but compared with the method of parallel-connecting, in which wire of half the sectional area was used in addition to the connections, a great gain was effected by the space occupied by the insulation of the wire being substituted for the copper conductor itself. The field-magnets were constructed of heavy wrought-iron bars without pole-pieces; there were four poles between which the armature rotated. The back or pulley-end bearing was extremely long, coming close in on to the armature, thus carrying it rigidly. The results of tests of this dynamo, which was of a medium size, showed that for each volt of pressure a length of Mr. Andrews.

Fig. 43.



Andrews Dynamo-Electric Machine.
Scale $\frac{1}{12}$.

3 feet of wire was required on the armature, and a velocity of

Mr. Andrews. 3,000 feet per minute. The armature was $19\frac{1}{2}$ inches in diameter, and at the above velocity would make 600 revolutions per minute; whereas a two-pole machine with an armature, say 12 inches in diameter, would have to make 1,000 revolutions per minute to produce the same result.

Prof. Ewing. Professor J. A. Ewing desired to draw attention to a Paper by the late Professor Fleeming Jenkin, published in the Transactions of the Royal Society of Edinburgh,[1] where some of the points which had been dealt with in the Paper on "High-Speed Motors" were very fully treated. Professor Jenkin had made a complete and rigorous analysis of the forces in a reciprocating steam-engine of practical proportions, running at two widely different speeds, and had given curves of crank-pin effort which showed the exact influence of the inertia of the reciprocating-parts, and also the influence of friction at the joints, under various conditions of load and speed. He had also directed attention to the fact that, with an early cut-off, the inertia of the reciprocating-parts caused the effort to be much more uniformly distributed than it would be if there were no inertia; and he had further found that for the same reason the frictional efficiency of the mechanism in a high-speed reciprocating-engine might even be greater than in the same engine running slow, with the same steam-diagram in both cases. In Mr. Imray's calculation, the only inertia taken account of was the resistance to acceleration in the direction of the piston's motion, namely, the translational resistance of the piston and connecting-rod. But, besides this, there was the resistance to angular acceleration on the part of the connecting-rod, and also the "centrifugal force" of the connecting-rod considered as rotating about the cross-head. Both of these were taken account of in Professor Jenkin's calculation, and the former was a very important element. For the translational acceleration the formula given in Professor Jenkin's Paper was identical with equation No. 8 (page 121) in the Appendix to the Paper on "High-Speed Motors." For a complete analysis of the forces on the connecting-rod A B (Fig. 44) it was convenient to regard the motion as consisting of translation, with velocity v, in the direction A C, combined with rotation about A, with angular velocity ω. Calling M the mass of the rod, the resistance to acceleration was a force through the centre of gravity G, parallel to A C, and equal to $M\frac{dv}{dt}$. The resistance to angular acceleration was a force through the centre of percussion

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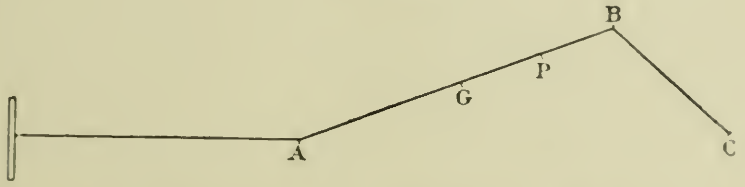
[1] Vol. xxviii. (1879), p. 703.

P, perpendicular to A B, and equal to $M \cdot AG \frac{d\omega}{dt}$. The "centrifugal force" was a force along A B and equal to $M \cdot AG \cdot \omega^2$. In Professor Jenkin's Paper, these forces were compounded, along with the weight of the rod, to form what he termed the "load" on the piece A B, and the crank-pin effort was then found by a graphic process which was fully described in his earlier Paper "On the Application of Graphic Methods to the Determination of the Efficiency of Machinery."[1] Prof. Ewing.

He thought that one of Mr. Imray's statements was liable to be misunderstood as an over-statement of the equalizing effects of the inertia of the reciprocating-masses. He referred to the sentence on page 115: "Obviously the inequalities that are left for the fly-wheel to subdue are the parts immediately above and below the inclined line K M L" (Fig. 1, page 114). This was true only if the rate at which the engine did work on the machinery it was driving was uniform per inch of travel of the piston, as was nearly the case in direct-acting pumps and blowing-engines. But generally the rate at which work was done by the engine on external machinery was nearly uniform per inch of travel of the crank-pin; and in that case the equalization of piston-effort which Mr. Imray spoke of still left a large margin to be done by the fly-wheel. Before it could be said how much energy had to be alternately absorbed and given out by the fly-wheel, it was, of course, necessary to draw curves showing the demand as well as the supply of energy, by setting up lines to represent driving-effort and resistance, at various points either along the piston-path, or what was generally more convenient, along the crank-pin path. It had been pointed out in the Paper on "High-Speed Motors," that one of the advantages of compounding engines, (namely, that a compound-engine, even if arranged with tandem-cylinders, gave less variation of piston-thrust throughout the

FIG. 44.



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[1] Transactions of the Royal Society of Edinburgh, vol. xxviii. (1877), p. 1.

Prof. Ewing. stroke, than a simple engine working with the same total expansion and power), was less conspicuous in fast than in slow engines, on account of the fact that in the former the inertia of the reciprocating-parts might equalize the piston-thrust sufficiently, without compounding. Another aspect of the matter, and one which strengthened Mr. Imray's contention, was this: that in a high-speed engine the cylinder condensation was less than in a slow engine, with the same range of cylinder temperature, and so the reduction of this condensation, which formed the main motive for using compound-engines, was rendered less necessary. The two considerations combined to make it exceedingly doubtful whether the compounding of very high-speed engines brought with it any benefit sufficient to compensate for the increased complexity and risk of failure.

Mr. Halpin. Mr. Druitt Halpin, with regard to reciprocating-parts, considered that in practice it would be wrong to put the weight in the piston, as this would, in horizontal-engines, produce great friction on the bottom of the cylinder, and corresponding wear of the rings. It must be borne in mind that the bottom of the cylinders was a part of the engine that could only be indirectly and inefficiently lubricated. If, on the other hand, the weight was put in the cross-head, the wear due to the weight could be easily taken up, by the special means provided for that purpose, and the friction in these parts would be much less, as the surfaces were capable of effectual lubrication. Transferring the blow from the piston to the cross-head would not produce any disadvantage, since this blow was transmitted, whether acting in tension or compression, on the short stiff pillar formed by the piston-rod, and the whole of the blow could be absorbed in the mass of the cross-head before it acted injuriously on the first joint of the mechanism consisting of the cross-head pin. In making use of the mass of the reciprocating-parts to produce greater uniformity of pressure on the crank-pin, it must not be forgotten that the conditions of lubrication of this pin were to a considerable extent altered. If the pressure on the pin was great at the commencement of the stroke from the pressure on the piston, and if the weight of the reciprocating-parts was kept light, and expansion took place in the cylinder, the oil had a chance of getting at the crank-pin, while the lighter pressures were in force; but if the pressure was kept uniform, owing to heavy reciprocating-parts, the lubricating surfaces would have to be proportionately increased. Taking a well-known extreme case to prove this, it was easy to see that the pressure on the crank-pin of an outside-cylinder locomotive was much greater, per unit of surface, than the pressure on the journal of the driving-axle, in this same engine. That these parts worked satisfactorily under such dissimilar pressures, could only be accounted for by the fact that in one case the pressure was intermittent, and in the other case it was continuous. Mr. Halpin.

Mr. Harrison. Mr. HUGH ERAT HARRISON observed that one of the great difficulties experienced, by those who were not the inventors, was to discover in what way modern dynamo-machines differed from their prototypes. The progress that had been made was due, almost entirely, to a proper dimensioning of their parts, which experience and experiment had shown to be advisable, and also to certain mechanical improvements from time to time introduced; whilst scarcely any device that merited the name of a novelty had been added. Important as were the mechanical changes, there was no reason why they should not have been embodied in the original machines, if only the designs, after leaving the hands of an electrician, had first been submitted to any practical mechanical engineer. Assuming the best materials to be used in all cases, the proper way of comparing dynamo-machines was to bring together machines of the same type so as to see clearly the effect of altering the proportions. A systematic study of dynamo-machinery needed a scientific classification based on something more than the names Pacinotti, Gramme, Schuckert, and others, which implied distinctions that did not exist. One way of classifying armatures was to arrange them according to the motion of the bobbins of the armature relative to the shaft of the machine. By a bobbin was meant any continuous portion of wire on the armature, included between two segments of the commutator. By the axis of a bobbin was meant a line perpendicular to its plane, and passing through its centre of symmetry. Taking those armatures which were modifications of the Gramme or the Pacinotti ring, it would be seen that they might all be classified as those in which: I, the axis of rotation was perpendicular to the axis of the bobbin, and did not intersect it. II. In armatures of the Siemens type: where the axis of rotation was perpendicular to the axis of the bobbin and intersected it. Mr Kapp had omitted that type of armature which was used for alternating-current machines, and for some direct-current machines, such as the Arago-disk machines. In these: III, the axis of rotation was parallel to the axis of the bobbin. The above classification was not exhaustive, but merely indicated what might be done.

Following in the steps of Reuleaux, with respect to steam-engines, Mr. Harrison. the above types of armatures might be represented by symbols, as in Figs. 45, 46, and 47.

Where *a x* represented the axis of rotation and B a bobbin.—Such arbitrary terms as Gramme ring, long cylinder, short cylinder, might be dispensed with by writing in the relative values of *a* and *b*. If it was desired to indicate that the armature had Pacinotti projections it could be done by the arms shown in Figs. 48 and 49.

Such a system at least had the advantage of giving credit to the originator of a particular type of armature, and not to the manufacturer who had merely altered the relative values of *a* and *b* in the above figures. In a short communication it was only possible to indicate what might be done in this direction. But if the same sort of classification were made for the field-magnets of machines, every machine could be completely defined by two symbols, one for the armature and one for the fields with the essential dimensions written in the figures. That such a classification was needed was evident from the fact that every one who made dynamo-machinery a special study began by producing a classification of his own.

FIG. 45.

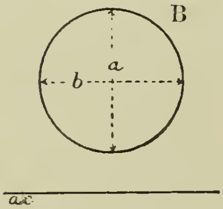


FIG. 46.

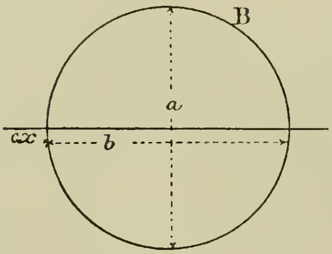


FIG. 47.

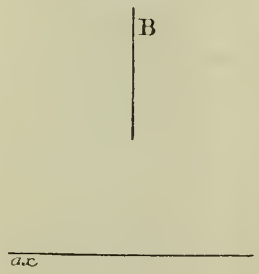


FIG. 48.

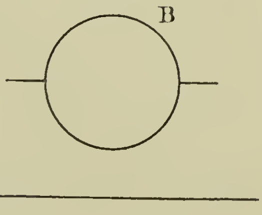
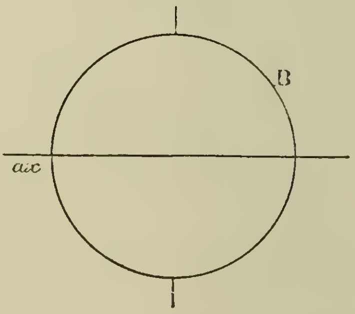


FIG. 49.



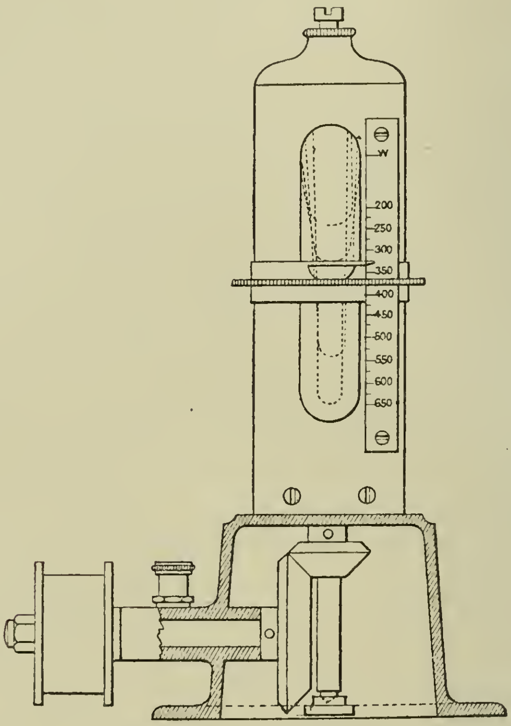
Mr. Hedges. Mr. KILLINGWORTH W. HEDGES observed that, having had some experience with both ordinary double-acting and single-acting rotary-

Mr. Hedges.

engines for driving dynamos, he thought a hard-and-fast line could not be drawn as to their respective merits, as each type was specially suitable for certain work. For instance, when designing several arrangements of portable tackle, comprising boiler, engine, and dynamo combined, on a base-plate on wheels, he had found it impossible to use direct-acting rotary-engines. One reason for this was that the rigid connection between the dynamo and the engine rendered the former liable to damage when passing over the rough ground often met with in docks, and other places where this portable lighting-gear was used. Apart from this, the strain set up in the shaft of a dynamo rigidly connected to the engine was very unequal, and probably was a source of loss of power, in that it was necessary either to have a flexible connection, or very accurate adjustment, to avoid excessive heating of the coupling, and probable shearing of bolts. On board ship, rotary-engines had a decided advantage, but he considered it better to run them at a moderate speed, and to drive the dynamo by a belt. High-speed engines gave trouble from the fact that, to maintain the speed, the lubricating arrangements were more complicated. In an instance which recently came under his notice, the electric light was stopped through the failure of a sight-feed lubricator of the rotary-engine, which prevented the high speed being maintained. He exhibited a new simple form of speed-indicator which he had designed for use, with machinery driven at speeds too high to be counted in the usual manner. This indicator was an improvement on an old form of liquid speed-gauge, and showed the number of revolutions by the depression of the liquid in a tube at the centre when rapidly revolved. The speed was read off on the case of the instrument by a brass ring furnished with two wires, which were slid to such a position that they aligned the point of the cavity in the tube. From Fig. 50 it would be seen that the instrument consisted of a cylindrical glass tube, sealed at the top and bottom, and partly filled with transparent fluid, which was not congealed by cold. At the lower end it was mounted on a spindle, while a cap at the top carried a pivot running on an adjustable centre, which could be set up to compensate for wear. Upon the spindle was a bevel-wheel gearing, with a pulley which was driven by a strap from the dynamo-spindle or engine-shaft, at the end of which was the main driving-pulley. The glass tube was covered with a metal case, pierced with a pair of slots for sight holes, and carrying a scale. When the tube was put in rapid rotation, the edges of the upper surface of the liquid crept up the glass, while the centre became correspondingly de-

Mr. Hedges. pressed, a parabolic cavity being formed which was exceedingly definite. The point of this cavity was the index, and its position could be exactly aligned on the scale by sighting it with the aid of a band which could be slid up and down the external case.

Fig. 50.



Hedges Speed-Gauge.

Scale ¼.

As shown, the indicator would record any speed between 200 and 650 revolutions per second. If required to have a more open scale, capable of registering an alteration of 1 or 2 revolutions, it was easily arranged by driving the indicator at a higher speed than was registered on the scale, which would show any change in the speed, with a larger depression for a fewer number of apparent revolutions.

Dr. E. Hopkinson. Dr. Edward Hopkinson remarked that the two types of dynamos manufactured by Messrs. Mather and Platt had been alluded to, namely, the Edison-Hopkinson and the Manchester dynamo. In the former of these machines theoretical principles had been followed probably more closely than in any other dynamo, whereas in the latter they had been subordinate to some extent to mechanical and commercial considerations; hence the comparison between them was instructive, as showing to what extent there was loss of

Dr. E. Hopkinson.

efficiency and power by deviation from the purely theoretical lines. The former machine, based on the Hefner-Alteneck principle of a drum-armature, had undergone improvement in respect to the arrangement of the magnetic circuit and construction of the armature-core by Edison, and finally developed into its present shape and proportions by Dr. John Hopkinson. In the original Siemens machine the magnetic-circuit was double, which, as pointed out by Mr. Kapp, necessitated a larger amount of copper in the field-magnets to produce the same induction per square centimetre, and a consequent increase in the power required to produce the field, and an increase in the cost of the copper. Edison, in some types of his machine, avoided this, but not uniformly, as even in some of his latest machines the magnetic-circuit was divided. Probably the reason which led him to this course was the idea that, by subdivision of the field-magnets, the effect of induction of the armature on the fields would be diminished; but it was certain that this effect was exceedingly small and might be neglected; hence all the machines of the Edison-Hopkinson type had a single undivided magnetic-circuit. Mr. Kapp had adopted ring-shaped magnets in order that the curvature of the lines of force might be continuous. This form of magnet was costly in labour, both in regard to forging and to winding, and as yet there were no experimental data to show that the magnetic resistance was diminished by avoiding abrupt turnings in the lines of magnetic force, and the reduction in length was of small moment, as its effect might be neglected in comparison with the resistance of the air-space.

In the winding of the magnets, the length and the section of the wire and the load depended upon quite other considerations than those which should obtain in regard to the armature. In shunt-wound machines, for a given section of iron and a given electromotive force, the section of copper was determinate, and not dependent upon the number of convolutions, and this latter was dependent entirely upon the amount of energy which it was allowable to use in producing the magnetic-field. The determination of it was exactly the same problem as the determination of the proper section for conductors. Consequently Sir William Thomson's well-known rule applied, that the cost of the power absorbed must be equal to the interest on the copper. Hence it was open to dynamo-makers to increase indefinitely the efficiency of their dynamos in respect to the waste in producing the field, by increasing the length of wire and consequently the original cost of the dynamo. In the Edison-Hopkinson dynamos the cost of

Dr. E. Hopkinson.

magnetization did not usually exceed 3 per cent. of the power developed. The rule promulgated by Mr. Kapp, regarding the extent of cooling-surface required per watt transformed into heat in the armature wires, was confirmed by the results of the continuous run for fourteen days of an Edison-Hopkinson dynamo, with a load of 55 volts and 275 amperes. The temperature of the armature attained its maximum after twelve hours' run. The area of the cooling-surface was 330 square inches; and the power transformed into heat in the armature 406 watts, giving 0·8 inch per watt, as stated in the Paper.

In the "Manchester" dynamo, made by Messrs. Mather and Platt, the arrangement of the magnetic circuit had been based largely on mechanical and commercial grounds. The magnetic-circuit was double for the sake of symmetry, and the magnets were placed on each side of the armature. This, while having the advantage of keeping the centre of gravity of the machine very low, and the space occupied as small as possible, necessitated yoke-pieces of considerable length, hence to some extent increasing the magnetic resistance of the circuit. The yokes requiring to be of a shape unsuitable for forging were made of cast-iron, and the section was increased, so that the resistance to magnetization was not less than in the wrought-iron magnet limbs. The resistance to magnetization in the several parts of the circuit were approximately proportional to the following numbers: Wrought-iron limbs, 29; armature, 40; yokes, 48; air-space, 220. Hence the use of the cast-iron yokes did not increase the resistance to magnetization more than 15 per cent.

A point of great importance between armatures of the drum- and of the cylinder-type was the effect of the convolutions in displacing the neutral point. In the Edison-Hopkinson dynamo this displacement was very small, and the intensity of the field, when the full current was passing through the armature was generally not more than 1·5 or 2 per cent. less than when no current was passing, the magnetizing force on the magnets remaining the same. Now in the cylinder-type of armature the displacement was considerable, and the diminution in the field at least 10 per cent. This effect had to be added to the fall in potential due to the armature-resistance, and these together caused a further diminution in the electromotive force, by reason of less current passing through the shunt-coils of the magnets. The difficulty was overcome by compound-winding or other equivalent devices. The result of deviation from theoretical principles was shown in the Table (p. 151), in which the Edison-Hopkinson dynamo stood at the head of

Dr. E. Hopkinson.

the list with only 18·2 inches of conductor in the armature per volt; the Manchester dynamo, though only second in the list, had 34·8 inches. The diagonal engine of Messrs. Mather and Platt had been designed chiefly from the point of view of economy of floor-space. The form of bed could be made very rigid, and also permitted the use of a fly-wheel of large diameter without cutting a wheel-trough. The stroke was short, so that the speed might be considerable, which enabled a high speed to be attained on the dynamo without unduly diminishing the size of dynamo-pulley.

Hon. Charles Parsons.

The Hon. CHARLES A. PARSONS remarked that the economy in the use of electric generators, where the motor was of the compound steam-turbine type, was one which was naturally asked, and had been referred to in the Paper on "High-Speed Motors." The largest sized generator yet made by Messrs. Clarke, Chapman, Parsons and Co., at the Victoria Works, Gateshead-on-Tyne, ran at a speed of 9,000 revolutions per minute, and gave a current of 200 amperes at 80 volts, with 50-lbs. steam-pressure, and on several occasions it had run for several days without stopping with this load. The generator consisted of a compound-turbine motor, coupled directly to the armature of the dynamo; both were mounted on bearings of special construction, and were lubricated automatically and continuously. The speed was governed by a combination of fan-governor and subsidiary electrical control, which kept the electromotive force to within 1 volt of the normal pressure. The magnets were shunt-wound, and took about 3 amperes of current. The armature was of very low resistance. The efficiency of this dynamo was therefore exceptionally high. The total weight of this generator complete was 15 cwt.; its steam consumption per electrical HP. was very moderate. No generator had yet been made at the Victoria Works expressly adapted to high pressure, and therefore they could not claim any very economical results. Experiments had shown, however, an economy not differing much from that of the usual types of engines and dynamos at present in the market working at the same pressure. The object first sought had been the construction of an electrical-generator suitable for use on board ships, where lightness, compactness, and simplicity were of the utmost importance.

Mr. Ravenshaw.

Mr. H. W. RAVENSHAW considered the Table of inches per volt, p. 132, was rather misleading; the results were hardly in accordance with practice. He had calculated the inches per volt, in the manner given by Mr. Kapp, for several armatures having the same total area, namely, 40 square inches, but of different proportions, and had found that, taking a circumferential speed of 3,000 feet

Mr. Ravenshaw. per minute, the speed of an armature, having a core of square section, was exceedingly slow, namely, 510 revolutions per minute, requiring a large number of turns to bring the output up to the required point; while an armature, of which the length of the core was twenty times the thickness, had an excessive speed, namely, 2,290 revolutions per minute, being far higher than was generally used. He had also found that, taking the proportion of core-diameter to thickness the same in each case, the results obtained were not in accordance with practice. The square core was of too large a diameter, and the very long core was too small to get the shaft and wire inside. It might be said that he had taken extreme proportions, but there were in the market cylinder-armatures having the length of core twice and eighteen times their thickness respectively. If, on the other hand, just sufficient room for the shaft, carrying arms, and air-space, was allowed inside the armature, the dimensions would be more in accordance with practice, and the inches per volt would be very different to the Table given by the Author, the square and the very long armatures being considerably less efficient than those whose proportions were from 2 : 1 to 5 : 1. These were shown by a curve, Fig. 24, which had been explained by Mr. Trotter, and it would be seen that the speeds varied from 750 to 1,475, instead of from 510 to 2,290 revolutions per minute. This, however, was a great range for the maximum speeds of differently proportioned machines of the same output, and it would be found that the speed of the machines of different makers, for the same purpose and output, did not vary more than from 10 to 15 per cent. It was hardly fair to compare machines of different outputs by the inches per volt, as the perimeter of a rectangle did not vary proportionally to its area, neither did the speed vary exactly in inverse proportion to the output. He had found that, for an armature having a core of 20 square inches total area, the inches per volt were 27; while for an armature having a total area of 200 square inches, the inches per volt were as low as 20·7, the proportion of the thickness to the length of the cores being as 5 : 1 in both cases. He had taken these figures from machines of the same design in actual practice. He believed that the best way of comparing machines of different makers on this point, was to take those having the same output, and to compare the inches per volt at the mean of their respective speeds. Long armatures had a considerable advantage over short ones in the reduced magnetic resistance; the notched armature, however, seemed to be the most convenient, as a low magnetic resistance could be obtained without having to use a long core, thus keeping the electrical resistance as low as possible. With respect to drum-armatures he thought the advantage of the shorter length of conductor was more than counterbalanced by the difficulty of repairing a burnt coil, in which case the whole armature would most likely have to be rewound. He doubted whether in practice the drum-armature had as great an advantage over the cylinder as Mr. Kapp made out; for the resistances of the modern cylinder-armatures were not so much higher than those of the drum-armatures in the best machines. From the assumptions by the Author, it appeared that the speed taken would be considerably higher than even that of the cylinder-armature in the case of the small core. Mr. Ravenshaw.

Mr. J. N. Shoolbred stated that Mr. Kapp pointed out in his Paper the advantages of those armatures, in the modern type of dynamo-machines, which were wound on the cylinder-principle, while he also indicated the value of the larger diameter of the disk-arrangement, in the diminished speed at which the dynamo had to be driven. There was a machine of an earlier date, a magneto, it was true, and an alternating one besides, where the advantages of the cylinder and of the drum types of armature occurred; but in a form differing considerably from those described in the Paper. This machine, the "De Meritens," was extensively adopted in lighthouse illumination, and with apparently the prospect of a still more extended use. The armature might be described as made up of a number of small armatures, each being wound on a solid iron core, which was itself built up of a number of thin wrought-iron plates. These small armatures were placed continuously, end on, round the periphery of a disk, but each was independent of the others, and was separated from its neighbours by a brass pin; thus forming, in the aggregate, a Gramme ring, but one which was divided into a number of segments, each magnetically disconnected from the others. The ends of the H-shaped cores of these little armatures stood up slightly above the level wire-coil which was upon the shank. These ends performed the part of the Pacinotti projections, and much of the efficiency of the armature was said to be due to this arrangement. The comparatively small amount of iron, as also of copper, in these small armatures (which were not merely made separately, but each of which was also readily accessible for repairs, without interfering with any of the others) must present advantages over such armatures of the disk type, as were in use with the Brush and the Gülcher machines. The excellent work being done by the "De Meritens" machines at the South Foreland was admitted in Mr. Shoolbred.

Mr. Shoolbred. the Report, on the Experiments on Lighthouse Illuminants which had lately taken place there.

The tendency with the modern type of continuous-current machines to increase the number of pairs of poles had also been pointed out by Mr. Kapp. It might not be generally remembered that in the earliest multipolar machines of the "Alliance" type, as far back as 1853, Mr. Holmes introduced devices to render the current continuous in direction. As, however, for lighthouse-illumination the alternating current was found to be suitable, these devices were later on dispensed with.

Much attention had of late been paid to the important question of the ventilation of the different parts of the armature, many of the attempts, however, being but improvements upon the form of the Gramme ring in this respect. It might nevertheless be interesting to mention one modification which appeared some years ago in the Heinrick's machine, and which seemed to promise well. The core consisted of soft-iron wire, coiled circumferentially upon a sort of channel-iron frame of cast-iron, thus forming a ring having a U-shaped groove in the interior. The copper wire, coiled transversely upon this core, had gaps at intervals to allow of the free passage of the air into the inner groove of the core. Another peculiarity of this armature was its mode of attachment by means of brass loops passing over the iron-wire core, and with their inner ends attached to the boss keyed on to the shaft. Thus the driving power was transmitted direct to the exterior of the armature, and at the same time any centrifugal tendency was controlled in a way which was not possible with the Pacinotti projections. It would be seen, therefore, that though much progress had been made of late in the modern type of dynamo-machines, as regarded their mechanical construction and the selection of the materials constituting them, yet, in the matter of design and arrangement, some of the earlier types still presented features well worth studying. The use of a characteristic curve of magnetization had been advocated by Mr. Kapp, to show the magnetizing power and the strength of field of a machine, instead of merely its current and electromotive force as in the older form of "characteristic" curve. Such a form, representing ocularly a measure of the output of the machine, and applicable equally to all classes, whether compound, shunt, or series-wound, must be more useful than the older forms.

With respect to the type of steam-engines best suited to driving dynamos, it seemed certainly strange to find Mr. Imray

Mr Shoolbred

advocating the return to single-cylinder engines, and also the abandonment of the compound principle, when the tendency, in the use of engines, was very decidedly the other way. The use of double-cylinder engines, whether compound or not, when driving dynamos by means of belting, had certainly become much more common than in the early days of electric lighting. Why should not this use of double-cylinder engines continue even with direct-driven dynamos, especially as the increasing size of the dynamo meant a diminution in its speed? It needed more than the mere assertion of the makers of single-cylinder engines to prove to the general public that such a step was in the wrong direction.

Mr. Walker.

Mr. Sydney F. Walker defended the old Gramme machine. Mr. Kapp had been very hard upon Mr. Gramme, although it would appear that the form of machine finally adopted by him was merely the old Gramme modified in certain points. Mr. Walker had been connected with the Gramme machine when it was first brought to this country, and had carefully watched the developments of every type of machine which had come into the market. When the advance of electric lighting, and the expiration of the patent warranted, the firm with which he was connected adopted the original Gramme machine, believing that though it might have faults, the faults were of structural detail, that could be easily got rid of by slight alterations, and not of principle; and that, on the other hand, it contained many excellent features, which were rarely all present in other dynamo-machines. It could easily be adapted to most of the improvements. The wooden hub, for instance, which appeared to be a rock of offence, was by no means such a bad mechanical contrivance for transmitting the power from the spindle to the wires, provided it was properly constructed. As originally arranged, when made in one piece, pushed in from one side, it did not answer well if much strain was brought upon it; but that plan had been given up for several years. By a simple modification the wooden hub had been rendered efficient, under as severe strains as Mr. Kapp appeared to think it wise to place on his more complicated arrangements. By simply dividing the hub into halves, the difficulty of transmitting the power from the spindle to the bobbin had so far been completely overcome that, for testing purposes, the bobbin had been running at a surface speed of 12,000 feet per minute for some hours without damage. It was, of course, necessary to see that the internal diameter of the bobbin was of sufficient size to give the wooden hub a proper grip, but there was no difficulty in arranging this. The outer wires

Mr. Walker. were also always well bound in with strong wire bands, so that the whole bobbin formed one solid body. On the other hand, he thought that, apart from the simplicity of the Gramme bobbin with its wooden hub, difficulties of insulation, and, in some cases, of heating, were encountered from which the former was entirely free. Metal radial arms, for instance, if used to carry the core, should be insulated from the core, otherwise they formed a connection between the parts of the core, and would give rise to heating and loss of electricity; and it was by no means always easy to accomplish this insulation. In the Crompton machine apparently it was not attempted. In the Elwell-Parker it was stated that the arms were tipped with vulcanized fibre, and through this small piece of fibre the whole of the power had to be transmitted. Bearing in mind that vulcanized fibre was a material which had a great tendency to split, and that it lost its insulating properties to a very large extent in the presence of moisture, this seemed to be absurd. He believed it would be found in every case, where an attempt was made to insulate the arms, that the insulation would in time break down merely from the vibration and the state of electrical strain always present. He admired the careful and ingenious way in which Mr. Kapp's armature had been worked out, but on examining the advantages claimed by him for it, compared with what had been done on the older and simpler plan, he was doubtful if the game were worth the candle. The current-density of 2,500 amperes per square inch was exceeded in machines made by Messrs. Walker and Olliver, and under conditions more severe than many machines were subjected to, namely, that of continuous work day and night, underground in collieries from early Monday morning to Saturday evening, with only a small precautionary rest twice a day for examination; and this went on from year's end to year's end, without any appreciable wear except at the commutator, and without a hitch so far as the dynamo was concerned. Mr. Kapp permitted a surface speed of 3,000 feet per minute, whereas the machines referred to above, ran continuously, without any sign of damage to the hub or wires, at a surface speed of over 4,000 feet per minute. The only point upon which he should have any doubts as to the advisability of abandoning the wooden hub, would be where it was advisable to increase the iron in the core very considerably. With an armature of a given diameter, the weight of iron could be increased in one way only, namely, by increasing its radial depth. With small diameters, of course, under no circumstances could this be carried beyond a certain point, owing to the necessity of providing for the accommodation of the wires on the inside of the core. Mr. Kapp had **Mr. Walker.** given the possible radial depth of core at one-fifth the diameter, taking the two opposite sides, or one-fifth the radius, taking one side. This afforded a depth of 1 inch for a 10-inch core, and left 8 inches as the internal diameter, in which provision had to be made for the driving-arrangements, and for the internal copper wires. In many cases this radial depth could be exceeded, but immediately beyond 1 inch there arose a difficulty in winding. As the radial depth increased, the difference between the external and the internal diameters of the core increased, and it followed that above a certain point, spaces were left on the external surface of the core which were not covered by wire, while the internal surface was continuous. When the internal was half the external diameter, these spaces would be equal to half the entire external surface. The question that here arose, and it seemed to be one for experiment, was, whether more was gained by the increased thickness of the core than was lost by having the smaller number of turns of wire? And, having these spaces, might it not be as well to reduce the magnetic resistance by filling them with iron, as in the Pacinotti armature constructed by Messrs. Paterson and Cooper? In theory the Pacinotti armature had the advantage; but apparently the theory had not taken account of the heating effect of the Pacinotti projections upon the pole-pieces of the field-magnets. As far as he understood, these projections entailed loss in one way or another, either in the heating of the pole-pieces, or in the higher speed, or diminished output, entailed by the increased clearance between the armature and pole-pieces. This was on the supposition that it was not possible to get rid of the heating effect on the pole-pieces in any other way, and without loss; but with the old Gramme pattern there was no difficulty in laminating the pole-pieces, just as the armature was laminated, if it should turn out that the Pacinotti form of armature was the best. This illustrated the point he had before mentioned, of the facility with which improvements could be adapted to the old Gramme machine. The point he wished to emphasize in connection with the increased depth of core, and the old Gramme bobbin with the wooden hub, was this, that if it should be found advantageous to go far in that direction, it would be necessary to abandon the wooden hub, because the small internal diameter would not give sufficient hold to it; but for the figures mentioned by Mr. Kapp, as the largest depth he had found it advantageous to use, he thought there would be no difficulty in arranging the winding to it, while retaining all the advantages of the wooden hub. Messrs. Walker

Mr. Walker. and Olliver were, in fact, constructing armatures with wire cores 1 inch in diameter, and with wooden hubs, without the slightest difficulty. The aim was to increase the output by increasing the amount of iron in the armature, provided sufficient lines of force could be produced to utilize this greater amount of iron. With a dynamo of certain dimensions, having field-magnets whose cores were of certain dimensions, there was a point beyond which any increased weight of iron added nothing to the output; and the question then arose, how far it effected the commercial efficiency of the machine to saturate the armature, or the reverse? He held, that to obtain the largest output from a dynamo of given size and weight, both field-magnets and armature-core should be charged up to a certain point, and that for efficiency both should only be magnetized up to a much lower degree. Another point of importance was that unfortunately the coils of armatures were sometimes burnt, particularly those wound with fine wires. His experience with the old Gramme machine had been that, in those cases where a single coil had been burnt, say, by an accidental short-circuit in turning up the commutator, or from any other cause, the damage had been confined to that coil and to the adjacent one on each side, the latter being in many cases very little injured. This was true even in many cases where the current passing through the whole bobbin had been excessive. In a Pacinotti armature which recently passed through his hands, two coils only had been burnt in the ordinary way, but twelve coils were injured; and these latter were apparently damaged by the heating of the iron core. Apparently one or two coils had been short-circuited and burnt. The heat from them was communicated to the iron core, which in its turn damaged the insulation of the other ten coils. The striking feature in the case was, that the injured coils were those included between two of the radial arms, by which the power was transmitted from the spindle to the core. In further reference to the old Gramme bobbin, he thought it would be admitted that no other form of construction was so simple or so cheap; and that in no other could the insulation of the copper wires from the core, and of the core from the spindle, be so easily and absolutely secured as in this much despised one. He should like to ask Mr. Kapp how the number of lines referred to were measured? and how he arrived at the figures for the length of wire per volt of electromotive force in the different cases mentioned? Moreover, he questioned the use of cast-iron for series-machines in place of wrought iron. Though the former had a low saturation point, more work had to be done upon it to produce

Mr. Walker.

the same results that would be obtained from wrought-iron, and more of it must be used in the construction of a machine; so that he thought it would be the cheapest plan to use wrought-iron and put in a smaller core. It was more easily worked, it would be lighter, and it did not often give rise to the troublesome faults that occurred with cast-iron, owing to flaws in the casting. He should point out too, that Mr. Kapp had been unjust to the earlier dynamos in his statement, by implication, that they were constructed with cast-iron field-magnets. Both the early Gramme and the Siemens field-magnets were made of wrought-iron, and in his opinion a step backwards had been taken by Mr. Brüsh and Mr. Bürgin, Levey and Lumley, and others, in constructing field-magnets of cast-iron. Every one who had been engaged in the construction of electrical apparatus knew that cast-iron was not as good, in any respect, as wrought-iron; and, further, that it made a great difference in the working of the apparatus whether wrought-iron of an inferior brand was used, or such as Low Moor or Farnley. Mr. Kapp had mentioned that it made no difference to the output of his dynamo where the magnetizing coils were placed, and he referred to the "Manchester" dynamo as a case in point. Mr. Walker would ask whether in the case of his own dynamo this was not due to the increase or decrease of the magnetic resistance being practically inappreciable, owing to the mass of wrought-iron? Whether, in the case of the "Manchester" dynamo, the good results obtained were not due to the comparatively low magnetic resistance of the large mass in section and short length of the pole-pieces? And whether he would expect to obtain the same result as was now produced by substituting for the massive pole-pieces, supposing mechanical arrangements would allow it, of a thin plate of cast- or even of wrought-iron, made in the shape of the pole-pieces, and connecting the ends of the magnets? He entirely agreed with Mr. Kapp with reference to the advantage of low-tension over high-tension machines, where the area was limited. For small installations he had gone still farther; and he had been enabled to run arc- and glow-lamps together from one machine, without any variation in the light given by the latter when the former fed, and without any of the complications necessary where series-machines were used. It had enabled Messrs. Walker and Olliver also to use an arc-lamp, which was as simple as arc-lamps could be made, in which motion had to be given to the carbons. The lamps, both arc and glow, burned with great steadiness; the latter were, in fact, absolutely steady, and each individual lamp was independent of the others.

Mr. Wingfield. Mr. C. H. WINGFIELD remarked that in using an indicator-card, for estimating the varying effort produced by the pressure of the steam on the piston of an engine, it was usual, he believed, to assume that the data for one stroke could be obtained from the diagram taken from the end of the cylinder which was receiving steam during the first portion of that stroke. For instance, in Fig. 51 (p. 273), where the two diagrams were about equal in area, it would be assumed that the same indicated HP. was being transmitted to the piston during both the up- and the down-strokes, and that during the down-stroke (supposing the cylinder to be vertical) the effective pressure on the piston, when it had moved $\frac{\text{A C}}{\text{A B}}$ of the stroke, would be *a b*, whereas it was really *a c*. Considering first the down-stroke, it was evident that the impelling force, due to the pressure of the steam on the top of the piston, was represented by the ordinates to the curve *f a d* on the card from the top end of the cylinder. The pressure tending to oppose the downward motion was the back-pressure on the other side of the piston, and was measured, by the ordinates to the exhaust line *g c e d*, on the card from the bottom end of the cylinder. Deducting one from the other, the result was as shown in Fig. 52, in which the length *a c* of a vertical line drawn through any point C of the stroke gave the true impelling force when the piston had moved through a part A C of the stroke A B. Taking the bottom card in the same way, the true card (Fig. 53) was obtained, which was very different from the original. It would be noticed that the impelling force gradually diminished, until at *b*, where the steam-line of the bottom card crossed the compression-curve of the top card, the steam-pressure was exactly balanced by the back-pressure, and the effort became nil. To the left of *b* the effort was negative, and tended to stop the upward motion of the piston. It now appeared that, whereas in Fig. 51 cushioning was shown at both ends, there was really none at the end of the down-stroke, the right-hand end of Fig. 52, and that that at the end of the up-stroke acted for a much shorter portion of the stroke, namely, from *b* to *f*, than would sometimes be supposed. Of course Figs. 52 and 53 were the correct diagrams to use in estimating the varying effort which was to be equalized by the inertia of the moving parts of the engine. If only the mean indicated HP. was desired, it was correct to measure the area of each of the diagrams in Fig. 51 in the usual way, and to take the mean of the two as a basis of calculation. If, however, it was desired to compare the work done in either end of the cylinder, the area of Figs. 52 and 53 must be taken as measures of the work in the top and bottom ends of the cylinder respectively. In the latter Mr. Wingfield.

FIG. 51.

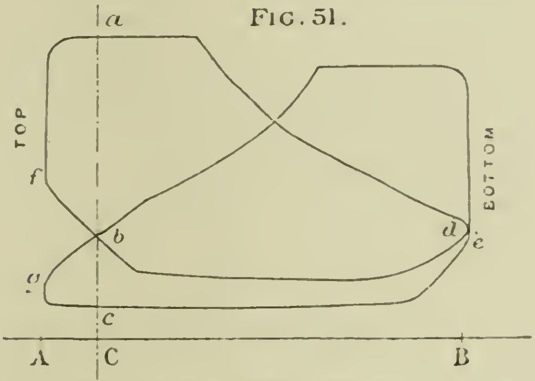


FIG. 52.

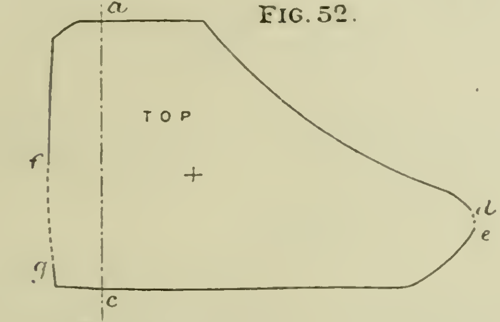


FIG. 53.

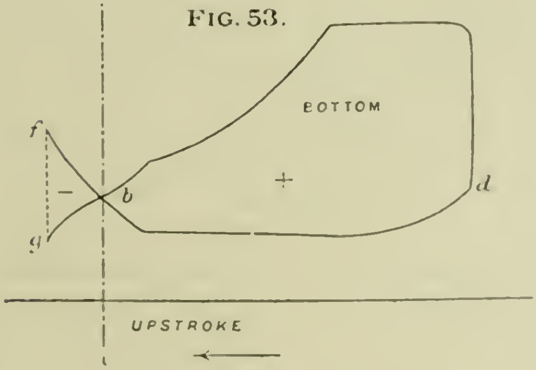
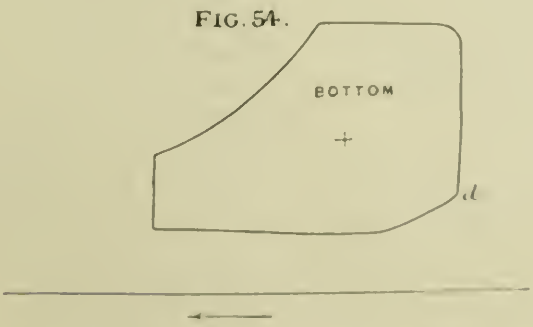


FIG. 54.



case the part marked – must be deducted from that marked +, Mr. Wingfield. leaving Fig. 54. The work done during the down-stroke, Fig. 52, was evidently very much greater than that performed in the up-stroke Fig. 54, though the mean of the two was the same as before, and yet the cards in Fig. 51 were about equal. Where very high pressures were used, the difference in the areas of the two sides of the piston, due to the area of the piston-rod, should be taken into consideration in calculating the effort.

1 December, 1885.

Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

The following Associate Members have been transferred to the class of

Members.

Arthur Brown.
Robert Davidson.
Walter Combermere Lee Floyd.
Stephen Horsley.
Richard Herbert Lapage.
Philip Townshend Somerville Large.
Hugh Unsworth M^cKie.
Reginald Empson Middleton.
Drewry Gifford Ottley.
Henry Strutt Collette Ree.
John Godfrie Single.
William George Strype.
Robert Henry Swindlehurst.

The following Candidates have been admitted as

Students.

Frederick Henry Addis.
Albert Evershed Anthony.
Earnest Wheatstone Baker.
William Timperley Batho.
Harry Oscar Beckh.
Robert Arthur Bell.
Hugh Osbourne Bennie, Wh.Sc.
William James Eames Binnie.
John Fuller Boake.
William Boggis.
Philip Bright.
Sydney Linton Brunton.
George Cunningham Buchanan.
William Edward Buck.
Gilbert Clark.
Charles Stanley Clarke.
Edmund Clarke.
Edward Clarke.
Ernest Henry Beales Claydon.
John James Davidge Cleminson.
William Henry Collis.
William Corin.
Joseph John Croyle.
Leopold Cust.
Miles Leonard Davis.
Frederick Dore, Jun.
Frank Noel Dymond.
Sydney Charles Early.
William James Edwards.
Bertram William English
Giulio Festa.
John Edward Fitt.
Harry Fitzmaurice.
Lamorock Flower.

Students (continued).

Guilherme Henrique Fox.
Alfred Bonner Gale.
Thomas Glover, Jun.
Henry Holmes Gray.
Albert George Hadcock. Wh. Sc.
John Hale.
Arthur Harrison.
Octavian John Alfred Harrison.
Thomas Francis Harvey.
Herbert Francis Hayllar.
Arthur Hayne.
Sidney Bedford Beves Hebb.
Edward Campbell Herbert.
David John Highet.
Ernest Frederick Wallis Hoare.
Charles James Hobbs.
William Hogg, B.E.
Joel Henry Horwood.
George William Humphreys.
Edward Hunt.
Maurice Hunter.
Arthur John Ikin.
Charles Herbert Innes, B.A.
Frederick Newcome Jones.
Hugh Troubridge Keeling.
King Kiku Kingdon.
Henry Littlejohn.
Albert Ernest Lott.
George Champion M^{c}Donald.
Benjamin Marsland, Jun.
Charles Mayne.
Edward Alworth Mitchell Merewether, B.A., B.E.
Charles Murray.
William Thomas Nye.
Ernest Moncreiff Paul.
Harold Charles Paxon.
William John Peace.
Frederick Bernard Pemberton.
Leonard Penson.
Charles Henry Dillon Pettigrew.
John Morgan Price-Williams.
Alexander Forbes Proctor.
Frederick Harvey Rapley.
Edward Walter Rathbun.
Henry Ashman Reed.
Henry Fines Richards.
Charles Hugh Romanes.
Arthur Bretherton Rouch.
Ernest Sydney Rundle.
Evelyn Francis Sanders, B.E.
William Cross Sellar.
Archibald Sharp, Wh. Sc.
Harry Ross Skinner.
Bertram Robert Smith.
Roger Thomas Smith, B. Sc.
George Herbert Sowray.
George Maynard Stainforth.
Frank Standfield.
Sydney Francis Staples.
James Melville Stevenson.
Alexander Stewart.
Henry Edward Stilgoe.
John Archibald Taylor.
Thomas Frame Thomson.
Charles Leslie Walker.
Charles Parry Warde.
Alexander James William Watkins
Sidney Herbert Wells.
Frederick Alfred Wilson.
Ebenezer Clarence Wood, B.A., B. Sc., B.E.
Ernest Arthur Boraman Woodward.
Evelyn Henry Young.

The following Candidates were balloted for and duly elected as

Members.

Robert Duncan.
James Gresham.
John Francis Hall.
Edmund Mills Hann.
Henry James Bennet Hargrave, M.A.
John Harrison.
Charles Mattathias Jacobs.
George Alexander Keefer.
John Landor Lowe, B. Sc., A.K.C.
James Platt.
Ithel Treharne Rees.
Albert Edward Seaton, Wh. Sc.
Howard William Warden.
Henry Hartley West.

Associate Members.

William Sutherland Abbott.
William Milward Allen.
François Albert Louis Alliman, Stud. Inst. C.E.
William Phillipson Anningson.
George William Heaton Armstrong.
Samuel George Herbert Barfield, D. Sc.
William Marrow Beckett, Stud. Inst. C.E.
Edward Benest Shaw Benest.
Joaquim Ben-Saude.
Benjamin Charles Bensley.
Richard Evans Willoughby Berrington.
Francis Trevor Bird, Stud. Inst. C.E.
Silvio Bonavia.
Henry Jean Breton.
Sydney George Brounger.
Arthur Kaye Brown, Stud. Inst. C.E.
Cyrus Bullock, Wh. Sc.
George Lewis Burton, Stud. Inst, C.E.
Daniel Campbell, Jun., Stud. Inst. C.E.
Athol John Capron.
Alexander Clark.
Francis Alfred Cooper, Stud. Inst. C.E.
John Cooper.
Charles Harvey De Mello.
Miguel Detsi.
William Aitken Duff, Stud. Inst. C.E.
William John Duffy, A.K.C.
George Eaton-Shore.
Gilbert Francis Eliott, Stud. Inst. C.E.
Edmund Colvile Elliot.
Sackville Fisher, Stud. Inst. C.E.
Edward Gifford.
Walter Giles.
Frederick Joseph Hadfield.
Jonathan Haigh.
James Cracroft Haller.
Edmund Lincoln Harper.
William James Hodgson.
Toshinori Ihda.
Ernest Allen Horsford James.
Mathew Buchan Jamieson, Stud. Inst. C.E.
Christopher Jobson.
James Jones.
Frederick Talbot Joyce.
George Bastable Laffan, B.E.
Alexander Maurice Lane.
Ernest Edward Light, Stud. Inst. C.E.
John Martin M'Currich, M.A.
James M^cKie.
Enoch George Mawbey.
Thomas Lodwick Miller, Stud. Inst. C.E.
John Charles Mills.
Charles William Morgan, Stud. Inst. C.E.
William Gÿllich Newton.
Frank Overton, Stud. Inst. C.E.
John Shield Pattinson.
Joseph Phillimore.
Henry Bolland Powell.
Edward Bellingham Price.
Herbert Barrington Pyne.
Thomas Ravenhill, Stud. Inst. C.E.
Frederick Lawrence Rawson, Stud. Inst. C.E.
William James Robinson.
Leathom Earle Ross.
Charlie Kilner Sheard.
Octavius James Shedlock.
Arthur Hindhaugh Shield, Stud. Inst. C.E.
James Campbell Smith.
Walter Alexander Smith.
Plotino Soares.
Harry Laurie Stannard.
Frederick Wilfrid Scott Stokes, Stud. Inst. C.E.
André Blasini Stollmeyer.
Walter Strapp.
Johann Philip Edmund Charles Stromeyer.
Francis David Topham, Stud. Inst. C.E.
James Edward Tuit, Stud. Inst. C.E.
John Henry Tudsbury Turner, Stud. Inst. C.E.
Sydney Ferris Walker.
Frederic Lumb Wanklyn, Stud. Inst. C.E.
Thomas Rawdon Ward, Stud. Inst. C.E.
John Joseph Whiteley.
William Willcocks.
Morgan John Manderstjerna Williams, Stud. Inst. C.E.

Associates.

José Joaquim de Carvalho Bastos.
Henry Bayley.
Alfred Carpmael.
Edmund Penley Cox.
Vincent Walker Hill.
William Charles Kernot.
Edward Croft Greenway Thomas.
Samuel Lucas Turner.

The discussion upon the Papers on High-Speed Motors and Dynamo-Electric Machines by Mr. Imray and Mr. Kapp occupied the evening.

8 and 15 December, 1885.

Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

The discussion upon the Papers on High-Speed Motors and on Dynamo-Electric Machines by Mr. Imray and Mr. Kapp was continued through both evenings and concluded.

22 December, 1885.

Sir FREDERICK J. BRAMWELL, F.R.S., President,
in the Chair.

(*Paper No. 2108.*)

"On Construction in Earthquake Countries."

By John Milne, F.G.S., Professor of Mining and Geology,
Imperial College of Engineering, Tôkiô, Japan.

As the result of observations chiefly made in the main island of Japan, the Author comes to the conclusion that in the whole of the Empire there is on the average at least one earthquake per day. In Tôkiô, where he has resided for ten years, there are usually from thirty to eighty disturbances in a year. Some of these are simply tremors, whilst others have been sufficient to overturn chimneys and to unroof houses. These facts show that the opportunities which residents in Japan usually have for studying the effects produced by earthquakes upon buildings are many. The buildings are of three types. First, ordinary brick-and-mortar structures, such as exist in Europe; secondly, light wooden houses of the Japanese; and thirdly, buildings strongly bound together with cement and iron rods, which are considered to be earthquake-proof. In addition to observing effects which have been produced upon buildings, the Author has, at various times, instituted experiments to measure the relative motion which takes place in different parts of a building when shaken by an earthquake. Other experiments have been made to determine how far earthquake-motion may be cut off from buildings.[1]

The following general description of the observations is divisible into two heads. The first treats of the precautions which may be taken to avoid or lessen the momentum of an ordinary building when shaken by an earthquake. This subject has never, hitherto, been investigated. The second treats of the methods best adapted to obviate the destructive effects consequent on that portion of the motion which cannot be avoided, and therefore shakes the building. Before entering on these subjects, an epitome of the facts arrived at, connected with earthquake-motion, is given, having a special bearing on construction.

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[1] An aseismatic joint invented by Mr. D. Stevenson, M. Inst. C.E., is described in the Paper on "The Japan Lights," by Mr. R. H. Brunton, M. Inst. C.E., in the Minutes of Proceedings Inst. C.E., vol. xlvii., p. 6.

Generally the earthquakes which have produced effects on buildings in Japan commence with tremors of small amplitude and short period.[1] These appear to be surface-waves or ripples, and they are probably the source of the accompanying phenomena of sound. After the tremors, which usually last ten or twelve seconds, comes the shock. If this has an amplitude of 25 millimetres (0·98 inch), and a maximum acceleration of 500 or 600 millimetres (19·7 or 23·6 inches) per second, brick chimneys are in danger of being cracked. The amplitude and period of a shock are measured from diagrams taken by seismographs. From these quantities, on the assumption of simple harmonic motion, the maximum velocity, which determines the projecting power, and the maximum acceleration or intensity, may be calculated. That this assumption is practically correct has been demonstrated by experiment.[2]

Those who are familiar with calculations respecting the intensity of earthquake-motion will perceive that the methods here pursued are different from those followed by the late Mr. Robert Mallet, M. Inst. C.E., who calculated the overturning, or shattering effects of earthquakes on the assumption that the movement was equivalent to a sudden blow. Although the Author has obtained many hundreds of earthquake-diagrams, and also many from explosions of dynamite and other bodies, he has not been able to obtain the quantities employed by Mr. Mallet. In an earthquake a body is overturned or shattered by an acceleration, f, which quantity is calculable for a body of definite dimensions.

In a diagram this quantity f lies between $\frac{v}{t}$ and $\frac{v^2}{a}$ where v is the maximum velocity, t is the quarter period, and a is the amplitude, or half semi-oscillation.

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[1] Memoirs of the Science Department, Tôkiô Daigaku. Earthquake Measurement. By J. A. Ewing, 1883, p. 63. Report of the British Association for the Advancement of Science, 1884, p. 243.

[2] Transactions of the Seismological Society of Japan. Vol. viii. Seismic Experiments, pp. 1–82.

The phenomenon terminates by a series of irregular vibrations resultant on the first shock, together with other shocks, usually two or three in number, which may succeed each other at intervals of a few seconds. The period of all the vibrations depends partly on the intensity of the disturbance, and partly on the nature of the ground. The concluding vibrations have periods of from 0·2 to 0·25 second. From these remarks it is evident that there may be a disturbance of very large amplitude which would produce no destruction whatever. With regard to the direction of motion, it may be said that at two neighbouring stations it is only the shocks which have similar directions. The motions are generally performed in ellipses, paths like the figure 8, spirals, and a complexity of directions too intricate to define. Small earthquakes, in which there is no pronounced shock, are without any definite direction.

All the motions yet spoken of are horizontal motions. The vertical component is relatively so small that it may usually be neglected. For details of the seismographs, and the tests to which they have been subjected, reference may be made to many papers in the Transactions of the Seismological Society of Japan. In the vicinity of an epicentrum, there is without doubt much vertical motion, but of this the Author has had no experience. It may, however, be concluded that the area of the anaseismic wave is relatively small, and if the effects of the horizontal shock can be nullified, much destruction may be prevented. The first portion of this remark is based upon the following considerations:—

1. The experiments of military engineers with regard to the limiting radius of a crater of explosion.

2. Upon the results of the Author's experiments with dynamite and other explosives, which showed that within 50 feet of an explosion direct waves had no existence, the motion recorded being due to a horizontal surface-wave the vertical component of which was very small.[1]

3. Facts collected by the Author in mining-districts, where one earthquake out of many has not been felt underground, although chimneys were shattered on the surface.

4. On the smallness of earthquake-diagrams taken in a pit as compared with diagrams of the same earthquakes taken on the surface.

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[1] Transactions of the Seismological Society of Japan. Vol. viii. Seismic Experiments, p. 9 *et seq.*

I. Avoidance of Earthquake-Motion.

Experiments have shown that earthquake-motion may be partially avoided either by making a seismic survey of the area on which it is intended to build, and then selecting a site where the motion is comparatively small; or by adopting free foundations, or using deep foundations.

Seismic Survey.—During the last eighteen months the Author has had at and near his house a series of earthquake-stations, none of which were more than 800 feet apart. The instruments at these different stations were all placed in similar positions, and fixed on the heads of stakes level with the surface of the ground. The depth to which the stakes were driven was about 3 feet. When these instruments are placed side by side they give results practically similar. To reduce whatever slight errors might have been due to the instruments, they have been, from time to time, exchanged between the different stations. Four instruments have been placed on soft ground; one is within a few feet of marshy ground; another, also on soft ground, is at the foot of a slope about 6 feet high, rising up to ground which is hard and solid. The remaining stations, with the exception of one which is in a pit, are situated on high, dry ground, upon which there are several heavy brick structures. At the time of an earthquake a pendulum is set free to swing across a cup of mercury. At each contact with the mercury a current from thirty-three Daniel cells is sent to each of the stations. At the first contact the current sets in motion at each of them a carriage carrying a smoked-glass plate, upon which the seismograph writes. At every subsequent contact a mark is made upon the moving plate. By this means it is possible, so far as time is concerned, to institute the most minute comparison between the records given at each station. The smoked plates with their records are subsequently varnished and then photographed. As a general statement of the results which have been obtained by these investigations, it may be said that had different observers been placed in this small area, which is only 10 acres in extent, each would have given a totally different account of the same earthquake, both as to direction, amplitude, period, maximum velocity, and intensity. The actual results obtained for a few of the earthquakes which have been recorded are given in Tables in the Appendix, where A, B, C, D, E, F, G, H, and J, refer to different stations. A, B, E, and F are situated on soft ground. C, D, and J are on hard and relatively high ground. G represents the records taken in a house of peculiar construction, and H is the station in a pit. Whether as regards the average results, or particular earthquakes, it will be found that both in regard to the projecting power as measured by the maximum velocity, or the overturning and shattering power as measured by maximum acceleration, relatively high and hard ground is much more suitable as a site for building than soft ground.

The amount of these differences has, in certain cases, been so great, that it might be inferred that in some earthquakes buildings at C and D would remain standing, whilst similar buildings at any of the other stations would suffer serious injury. The ruins left by earthquakes like those of Lisbon and Jamaica led observers to suspect that the amount of destruction in different parts of a city varied with the nature of the underlying rock. The experiments now described have given measures of these variations. They have also shown that these differences may be very great even on a small area where there is no marked alteration in the nature of the soil, and where the existence of such alteration would hardly be suspected from inspection.

Since this discovery the authorities of Tôkiô have discussed the feasibility of making a seismic survey of the whole city, or at least of those portions where it is intended to erect large and important buildings.

Some years ago the Author conducted experiments to determine the extent of the range of motion on high ground as compared with that experienced on low ground. The result obtained at Tôkiô showed that there was least motion on the hills. This rule appears to be reversed at Yokohama.

Free Foundations.—Mr. Mallet in his introductory sketch to Palmieri's "Eruption of Vesuvius in 1872," speaking of the Japanese lighthouses, says that he was consulted by Mr. Thomas Stevenson as to the general principles to be observed in their construction; "and those edifices have been constructed so that they are presumedly proof against the most violent shocks likely to visit Japan; not, perhaps, upon the best possible plan, but upon such as is truly based upon the principles I have developed."[1] This quotation probably refers to the so-called aseismic tables which carry the lamps. These are tables resting on spheres. Although the tables may have much to recommend them, yet, owing to the ease with which they were moved by causes other than earthquakes, they have one and all been dispensed with. The only structure standing on balls with which the Author is acquainted is a small building which he erected as an addition to his own house about two years ago. In a report to the British Association for 1884 he described it as follows:—"The building, which measures 20 feet by 14 feet, is constructed of timber, with a shingle roof, plaster walls and ceiling of laths and paper. The balls rest on cast-iron plates with saucer-like edges fixed on the heads of piles. Above the balls and attached to the building are cast-iron plates, slightly concave but otherwise similar to those below. From the records of instruments placed in the building, it would appear that at the time of the earthquake there is a slow back and forth motion, but that all the sudden motion or shock has been destroyed. Thus far the building, or rather its foundations have proved successful in eliminating the destructive element of motion."[1]

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[1] "The Eruption of Vesuvius in 1872," by Professor Luigi Palmieri, with notes, &c. By Robert Mallet, p. 43.

The balls referred to were 10-inch shells. Although the device somewhat mitigated the effects of earthquakes, the motion produced by walking, by the wind, and by other causes, resulted in effects much more serious than those due to ordinary earthquakes. To increase the rolling friction the Author next employed 8-inch shot, and after that 1-inch shot. The last attempt was to support the building at each of its six piers upon a handful of $\frac{1}{4}$-inch cast-iron shot resting on flat plates. By this means friction has been so much increased that the building stands solidly, and unless its free foundations were pointed out, their peculiarities would not be noticed. If moved by the application of levers, the building remains in the new position. Its behaviour at the time of an earthquake may be judged by reference to the diagram of the earthquake of the 12th of February, 1884, Plate 3. With the exception of two waves marked A and B, the motion in the chamber was practically nothing; whilst waves equivalent to A and B, together with many which are greater, repeatedly occur in all diagrams taken from the ground in the vicinity of the house. By reference to the Tables in the Appendix the maximum velocities and accelerations inside and outside the house may be compared. Calculating the average values of these quantities, the advantage to be gained by using free foundations is still more marked.

The Author has found by experiment that a shot of the kind used is cracked with a steady pressure of 0·8 ton. If still finer shot and in greater quantity could be employed at each pier, the resultant advantages might be increased.

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[1] Report of the British Association for the Advancement of Science, 1884, p. 248.

These experiments seem to show that light one-storied buildings, like bungalows, built of wood or iron, may be put up so that sudden horizontal motion of the ground shall be prevented from being transmitted to them, and at the same time they shall resist disturbing causes like those due to wind, equally as well as other buildings.

Deep Foundations.—Experiments with regard to the advantages to be gained by using deep foundations principally apply to heavy structures of brick and stone. The Author regrets that hitherto, owing to the want of means, he has not been able to complete all that he had in contemplation to carry out. So far the results have been as follow:—

At the back of the dwelling-house is a pit 10 feet deep, and about 4 feet wide. At the bottom of the pit is a natural hard earth. Here is placed a seismograph in all respects similar to those at the various stations, and similar to two others on the surface of the ground just above the pit.

The general result obtained from this experiment has shown that at the bottom of the pit motion is always very small. Five different earthquakes have now been recorded in the pit, and the instrument has always been in good working order. In the diagram of the earthquake of the 20th of March, 1885 (Plate 3), which was tolerably strong, it will be seen that the record of the motion in the pit was almost invisible, while that recorded at J, about 20 feet distant, was as 1 to 43. The maximum velocity at these two points was as 1 to 52, and the maximum acceleration as 1 to 82.[1] The importance of this discovery to those who have to build in countries subject to earthquake is obvious. It is the Author's intention to make comparisons between a solid foundation rising freely in a pit, a similar foundation connected with the sides of the pit by brushwood and a covering of earth, and an ordinary foundation. To these may be added experiments on the isolation of a piece of ground by means of excavations.

II. AVOIDANCE OF DESTRUCTION DUE TO THE ACQUISITION OF MOMENTUM.

Having by the application of one or all of the methods now suggested reduced the motion which would be received by a building erected in an ordinary manner, the next point is to indicate principles and to adopt rules by which the effects due to the unavoidable momentum may be reduced to a minimum.

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[1] That these ratios will hold for all other earthquakes is uncertain.

From what has already been stated regarding the nature of earthquake-motion, it seems that stresses and strains applied horizontally have chiefly to be dealt with, and not so much stresses and strains due to gravity. As an illustration of the meaning which it is intended to convey, consider an ordinary masonry arch. For vertically-applied forces this is stable, whilst for horizontally applied forces its stability solely depends upon the adhesion of the material which cements it together. The Author has examined many brick arches which have been cracked by earthquakes in Japan. One of the sets of arches examined formed a connecting-link between heavy brick walls. All of the arches were cracked across their crowns, the cause probably being the horizontal vibration of the walls which they connected. A like set of arches in similarly constructed walls at right-angles to these, and at the same time at right-angles to the principal direction of the shock which had caused the damage, was not cracked. Archways of this description ought at least to be protected by iron or wooden lintels. If archways are indispensable, they should curve into their abutments and not meet them at an angle, the angle being a point of weakness.

The results of an examination of three hundred and thirty similarly built brick structures in the streets of Tôkiô showed that the upper windows of the houses had flattish arches meeting their abutments at an angle. Out of one hundred and twenty-seven cracks in these arches, no less than one hundred and thirteen ran from the angle. Out of two hundred and fifty cracks in the lower arches, one hundred and ten ran down from beams which supported a balcony, and one hundred and forty from some portion of the arch, usually near the crown. Not a single arch was observed to be cracked at the springing when the arch curved into its abutment. Another interesting point was that the number of cracks in walls running north-east to those in walls running south-east was as 1 to 1·3, and it is from south-eastern directions that statistics showed the principal shocks to have originated. This again illustrates the instability of archways to stresses applied horizontally and parallel to their direction of span. As another illustration of a structure weak in resisting horizontal forces, the high wall of a factory may be taken, or a church tower, which contains a series of openings like windows and doors vertically above each other. These openings constitute a line of weakness, and the wall may give way here at the time of an earthquake, as was illustrated in July 1880 at Manila. Openings should not be vertically above each other; they might be in horizontal rows, those in one row alternating in position with those of the neighbouring rows.

Another important rule which must be kept in view is to avoid coupling together two portions of a structure which from their position are likely to have different vibrational-periods.

A remarkable example of the violation of this rule was to be seen in Yokohama after the earthquake of the 20th of February, 1880. A moderately high factory chimney was supposed to require support, and it was therefore connected by an iron band to the side of a neighbouring building. When the earthquake came, the band, instead of giving the chimney support, cut it in two.

Many similar examples of destruction due to difference in vibrational-period could be seen in the chimneys of almost every bungalow, which were shorn off at their junction with the roof. By themselves, either the chimneys or the roofs of the bungalows would have been secure; but when in contact it was evident that they had been mutually destructive. If it is unavoidable that parts of a building having different vibrational-periods should be united, it would seem advisable to connect them by bonds so strong that the various parts thus connected should be constrained to move as a whole. Whilst speaking on this subject a few words may be said about so-called earthquake-proof buildings, for which in America patents have been granted. An example of this type is the City Hall of San Francisco. The chief feature in these structures is that iron or steel rods pass from side to side, and from front to rear, through the walls at each floor, being secured at the ends by washers and bolts. There are also vertical rods. A method of construction very similar to this has been described by Mr. J. Lescasse,[1] and several structures of this kind are to be seen in Tôkiô and in Yokohama.

With the exception of some chimneys on one of these buildings which were shattered in 1880, the others have stood in a satisfactory manner. The same remark may be applied to ordinary strongly-built structures where the complicated system of tie-rods has not been employed. Without doubt the stability of these buildings has been largely due to their strength, which has caused their various parts to vibrate as a whole. Another element which has given them stability has probably been their deep foundations; and had the walls just above the foundations been free at their sides, the experiments previously referred to show that the amount of motion would have been considerably lessened.

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[1] Mémoires de la Société des Ingénieurs Civils, 1877, p. 212.

Among the experiments made to determine the relative motion of different parts of a building, a few were carried on at the Imperial College of Engineering, which is a heavy solid structure of brick and stone. One set of experiments was made upon the archways of two corridors. These arches have a span of 8 feet 3 inches, a rise of 4 feet 1 inch, and rise from brick abutments 1 foot 11 inches thick, and 7 feet 1½ inch high. The voussoirs of the arch are made of a light grey volcanic tuff, and have a depth on their face of 12 inches. The width of the wall between the arches is 4 feet 6⅞ inches. Across the springing-line of these arches a light deal rod was placed. One end of this was spiked to the wall; the other end terminated with a fine steel wire, resting on the surface of a smoked-glass plate, placed on a ledge at the top of the other abutment. If these two abutments approached to or receded from each other, a line indicating the range of motion would be drawn upon the smoked glass. A second record of motion was obtained on a glass plate fixed on the middle of the transverse rod, by a pointer hanging vertically from the crown of the arch. The result showed that in a severe earthquake there was sometimes a motion of from 1 millimetre (0·04 inch) to 2·75 millimetres (0·11 inch), the vertical movement of the crown being slightly in excess of the horizontal motion. In slight earthquakes there was either no motion in the arches, or else the different parts of the arch had practically synchronized in their movements.

For a more detailed account of these experiments, together with other observations, the Transactions of the Seismological Society of Japan may be referred to.[1] Another interesting set of observations was made upon the cracks which exist in several of the buildings at the College of Engineering. At the basement of the buildings, which are constructed of a volcanic rock (andesite), the cracks follow the mortar-joints; but when they come to the brickwork above, they run up and down through the whole structure, sometimes along the mortar-joints, but just as often through the bricks. Some of the cracks have a width of ¼ inch. Across several of them steel-wire pointers were placed horizontally; one end of the pointer was fixed to the brickwork, whilst the other end rested on the face of a smoked-glass plate, in a frame nailed to the wall. In a severe earthquake it seems certain that the difference in phase of the portions of the building at the two sides of a crack sometimes reached 2 millimetres. In slight earthquakes no records were obtained. In addition to these experiments, the ends of a large number of cracks were marked and dated. After a strong shake, many of them were seen to have grown in length. It seems, therefore, that portions of a building which are not likely to synchronize in their vibrational-period ought either to be strongly tied together, or else, by means of joints intentionally left during construction, to be completely separated from each other.

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[1] Vol. ii., pp. 32-38.

The evil effects consequent on overloading the upper parts of walls, roofs, and chimneys have been mentioned at considerable length by Mr. Mallet in his classical work upon the Neapolitan earthquake.[1] When a chimney with a heavy top is suddenly moved forwards, the upper part, by its inertia, tends to remain quiescent. The result of this, as with all other heavy superstructures, is to cause a fracture between the lower part which has been quickly moved and the upper part which has tended to remain quiescent. In Japan many chimneys have been shattered by these causes. The last chimneys inspected by the Author were those of the British Legation, which partially fell, and were otherwise ruined, on the 15th of October, 1884. These were rectangular in section, half a brick thick, and loaded at the top with a heavy head. Chimneys much thicker, and without the heavy cap, have now been substituted. In Yokohama experience has taught almost every householder to make his chimneys short, thick, and without heavy ornamental copings.

Weighty tile roofs act upon the supporting walls like heavy tops upon chimneys. As an attempt to obviate this, some of the roofs of the Imperial College of Engineering have been built so as to rest loosely on their supporting walls. Had they rested upon layers of cast-iron shot, the increased freedom and independence of the moving walls would probably have rendered them still further secure. In Manila the effects of earthquakes have caused the inhabitants to adopt corrugated-iron as a roofing-material. Walls may be rendered light in the upper parts by the use of hollow bricks. The advantages to be gained by using light superstructures as a means of mitigating the effects of vertical motion are evident. An additional precaution against this movement may be obtained by the use of vertical tie-rods, connecting the upper portion of a building with its foundations.

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[1] Great Neapolitan Earthquake of 1857. "The first principles of Observational Seismology." 2 vols. 1862.

Conclusion.

In the foregoing Paper reference has been made only to general principles; all details of construction have been left to the architect and the engineer. The Author would recommend that ordinary inexpensive dwelling-houses should rise from a solid wall, which itself has a foundation deep enough to reach hard ground. If the ground is soft, and therefore liable to considerable motion, the house might rest upon layers of cast-iron shot not larger than buck-shot. Wooden houses are usually objected to on account of their inflammability and their appearance. If angle- and sheet-iron be used as the building material, internal walls of wood and paper will be required. In a hot climate like Manila three ceilings with corresponding air-spaces are employed. Chimneys may be made of iron tubing. The dangers of fire may be reduced by using two tubes placed concentrically with an air-space between them. Before erecting heavy structures of brick and stone, much might be learnt respecting the nature of the proposed site by instituting a seismic survey. Such buildings ought certainly to have deep foundations, and if the basement had a lateral freedom the motion to which the building is exposed would probably be reduced. With regard to safety dependent on excavations or the contour of the ground, it must be remembered that if a building is only partially surrounded by openings like ditches, moats, steep valleys, and the like, it may be in greater danger than if such excavations did not exist. The reason of this lies in the risk lest the earth-vibration, approaching from the side not cut off, should make the opposite side, where the motion reaches the excavation, a free surface, which would then swing forwards like the last truck in an uncoupled train struck at the other end by an engine. The area of ground capable of being protected by ditches for a given earthquake has not yet been ascertained. Motion which is visible on a level is not visible at the foot of a bank 10 feet deep. At this lower level at the distance of about 100 feet the lost motion, however, reappears.

In the construction of a building the most important principles to be followed are:—First, to provide against horizontally-applied stresses; secondly, to allow all parts of the building with different vibrational-periods either to have freedom amongst themselves, or else to bind them securely together with long steel or iron tie-rods, especially at the floors and near corners, as corners of buildings often suffer in earthquakes. Thirdly, to avoid heavy superstructures. A light iron French roof on the top of a tower may be as ornamental as a heavy coping and roof of stone, and experience has shown that in an earthquake it is much the safer. Although the details of construction have not been entered upon, it is well to point out the insecurity of steeply-pitched roofs, which, if covered with slate and tile, may be destroyed, whilst neighbouring but flatter roofs remain secure.

So far as the Author is aware, the Local Government of Manila is the only one which, in its Building Acts, has made provision against dangers consequent on earthquakes. This was after the disaster of 1880 when the employment of light iron roofs was insisted on. If the Governments of earthquake-shaken countries like Italy and Spain framed building-laws, based on the results of investigations made in Japan and other places, the destruction of life and property so often consequent on these terrible phenomena might be considerably diminished.

The Paper is accompanied by three tracings and a photograph, from which Plate 3 has been prepared.

APPENDIX.

OBSERVATIONS ON EARTHQUAKES AT TÔKIÔ.

1.—*Number of Waves in Ten Seconds.*

1884–1885	A	B	C	D	E	F	G	H	J
March 25 .	22	18							
March 31 .	36	32	23						
April 6 . .	30	25	32						
May 6 . .	32	26	35						
May 11 . .	30	27	35						
May 19 . .	37	33	21						
May 19 . .	22	26							
May 30 . .	28	21							
May 31 . .	31	28							
June 11 . .	32	26	..	26					
October 24 .	36	30							
November 16	34	32	..	38					
November 21	36	35	..	38					
November 27	36	28	..	38					
November 29	26	18	At D too irregular to estimate.						
December 7	..	34	At D too small to estimate.						
December 9 .	24	..	..	24					
December 16	30	28	..	40					
December 23	..	26							
December 30	32	..	..	42	30 or 15				
January 2 .	..	26	..	26	40 or 12	40			
February 1 .	28	20							
February 4 .	30	30	..	..	24				
February 12	30	28	..	..	14	34	72	ripples	
February 27	32	32	..	..	36				
February 28	..	..	..	..	50				
March 12 .	30	26	..	..	18	..	48		
March 20 .	30	30	..	..	14	..	..	12	26
Average .	30	28	29	34	23 or 28	87	50	12	26

OBSERVATIONS ON EARTHQUAKES AT TÔKIÔ—*continued.*

2. *Maximum Amplitude of Waves in Millimetres.*

1884–1885	A	B	C	D	E	F	G	H	J
March 25 .	0·10	0·5							
March 31 .	0·1	0·14	0·05						
April 6 . .	0·3	0·8	0·1						
May 6 . .	0·4	1·0	0·1						
May 11 . .	0·3	0·9	0·1						
May 19 . .	0·07	0·15	0·04						
May 19 . .	0·1	0·2	0·2						
May 30 . .	0·05	0·1							
May 31 . .	0·05	0·1							
June 11 .	0·15	0·25	..	0·1					
Oct. 24 . .	0·07	0·1							
Nov. 16 .	0·25	0·3	..	0·05					
Nov. 21 .	0·1	0·25	..	0·05					
Nov. 27 .	0·15	0·25	..	0·05					
Nov. 29 .	0·2	0·6	..	0·05					
Dec. 7 . .	0·1	0·2	..	0·05					
Dec. 9 . .	0·07	..	..	0·05					
Dec. 16 . .	0·8	1·2	..	0·25					
Dec. 23 . .	..	0·1							
Dec. 30 . .	0·45	..	..	0·2	1·9				
Jan. 2 . .	..	0·25	..	0·05	2·5	0·05			
Feb. 1 . .	0·05	0·07	..	..	0·1	0·01	0·05		
Feb. 4 . .	0·05	0·1	..	..	0·05	0·02	..		
Feb. 12 . .	1·2	0·8	..	..	2·2	0·7	0·5		
Feb. 27 . .	0·1	0·12	..	..	0·05	..	0·04		
Feb. 28 . .	..	..	..	..	0·05	..	..		
March 12 .	0·1	0·3	..	..	0·6	..	0·1		
March 20 .	1·3	1·4	..	..	1·9	..	..	0·035	1·2
Average .	0·37	0·40	0·07	0·09	0·95	0·19	0·17	0·035	1·2

Observations on Earthquakes at Tôkiô—*continued.*

3. *Period of Largest Wave in Seconds.*

1884–1885	A	B	C	D	E	F	G	H	J
March 25 .	0·72	0·85							
March 31 .	0·30	0·24	0·33						
April 6 . .	0·36	0·61	0·36						
May 6 . .	0·47	0·70	0·36						
May 11 . .	0·35	0·47	0·26						
May 19 . .	0·23	0·36	0·20						
May 19 . .	0·40	0·50	..						
May 30 . .	0·36	0·40	..						
May 31 . .	0·35	0·30	..						
June 11 . .	0·36	0·36	..	0·36					
October 24 .	0·32	0·41	..	..					
November 16	0·47	0·47	0·23	..					
November 21	0·27	0·45	..	0·30					
November 27	0·45	0·36	..	0·20					
November 29	0·36	0·56	..	0·40					
December 7 .	..	0·24	..	0·24					
December 9 .	0·40	..	..	0·40					
December 16	0·45	0·54	..	0·37					
December 23	..	0·40	..	..					
December 30	0·32	..	..	0·18	0·75				
January 2 .	..	0·70	..	0·18	0·9	0·18			
February 1 .	0·45	0·82	..	..	0·5	..	0·18		
February 4 .	0·21	0·30	..	..	..	..	..		
February 12.	0·39	0·39	..	..	0·72	0·42	0·39		
February 27.	0·24	0·28	..	..	0·25	..	..		
February 28.	..	..	..	..	0·18	..	..		
March 12 .	0·18	0·29	..	..	0·64	..	0·31		
March 20 .	0·44	0·53	..	..	1·4	..	..	0·85	0·55
Average .	0·29	0·46	0·30	0·21	0·66	0·30	0·44	0·85	0·55

OBSERVATIONS ON EARTHQUAKES AT TÔKIÔ—*continued.*

4. *Maximum-Velocity of Wave in Millimetres per Second.*

1884–1885	A	B	C	D	E	F	G	H	J
March 25 .	0·9	3·7	..						
March 31 .	2·1	3·6	0·9						
April 6 . .	5·0	8·0	1·7						
May 6 . .	5·3	9·0	1·7						
May 11 . .	6·0	12·0	2·4						
May 19 . .	1·8	2·6	1·2						
May 19 . .	1·5	2·5	..						
May 30 . .	0·9	1·5	..						
May 31 . .	0·8	2·0	..						
June 11 .	2·7	4·5	..	1·8					
October 24 .	1·3	1·5	..	..					
November 16	3·5	4·0	..	1·3					
November 21	2·2	3·5	..	1·0					
November 27	0·2	4·4	..	1·5					
November 29	3·4	7·0	..	0·78					
December 7	..	5·0	..	1·2					
December 9	0·1	..	..	0·7					
December 16	0·11	1·4	..	4·2					
December 23	..	1·5	..	..					
December 30	0·9	..	..	7·0	1·6				
January 2 .	..	2·2	..	1·7	1·7	1·7			
February 1	0·7	0·6	..	..	1·2	..			
February 4	1·5	2·0	..	..	..	..			
February 12	1·9	1·3	..	..	1·9	10·1	12·0		
February 27	2·6	2·7	..	..	1·2	..	..		
February 28	..	..	..	..	0·2	..	..		
March 12 .	3·4	6·0	..	..	5·8	..	0·2		
March 20 .	1·8	1·6	..	..	0·8	..	..	0·25	1·3
Average .	4·4	5·3	1·6	1·4	8·7	5·9	7·0	0·25	1·3

OBSERVATIONS ON EARTHQUAKES AT TÔKIÔ—*continued.*

5.—*Maximum acceleration of Wave in Millimetres per Second (Intensity).*

1884–1885	A	B	C	D	E	F	G	H	J
March 25 .	3	27							
March 31 .	44	92	16						
April 6 . .	83	80	28						
May 6 . .	70	81	28						
May 11 . .	120	160	57						
May 19 . .	46	45	38						
May 19 . .	22	81							
May 30 . .	16	22							
May 31 . .	13	40							
June 11 . .	48	81	..	32					
October 24 .	27	22							
November 16	49	53	..	34					
November 21	48	49	..	20					
November 27	27	77	..	45					
November 29	57	81	..	12					
December 7 .	..	125	..	28					
December 9 .	14	..	..	9					
December 16	151	171	..	70					
December 23	..	22							
December 30	180	..	..	245 ?	135				
January 2 .	..	19	..	58	116	58			
February 1 .	10	5	..	..	14				
February 4 .	45	40							
February 12 .	300	210	..	..	170	145	128		
February 27 .	67	60	..	..	28				
February 28 .	..	..	..	..	80				
March 12 .	115	120	..	..	56	..	40		
March 20 .	249	182	..	..	34	..	..	1·7	140
Average .	75	75	33	35	79	101	84	1·7	140

[DISCUSSION.

Discussion.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, President, said that the Author of the Paper was still resident in Japan. The Paper was one of an unusual character, which fortunately did not interest Englishmen very much. Some gentlemen might be present from Colchester who remembered the scare created not long ago by an earthquake in that neighbourhood. He hoped that any gentlemen having personal experience of earthquake countries might be able to give the Institution the benefit of their observations.

Professor Perry.

Professor JOHN PERRY observed that although he had had some acquaintance with the earthquakes of Japan, he had no personal knowledge of the actual measurements, such as those which Mr. Milne had been in the habit of making. In 1876 Professor Ayrton and he, living as they did in Japan, and having an experience of a considerable number of earthquake shocks, felt it to be their duty to pay a little attention at all events to the investigation of the subject. The results of their study were published in a few Papers shortly after that date. Those Papers brought out chiefly a principle, which, in their opinion, had been until that time completely neglected by persons making observations on earthquakes in Japan and other countries; namely, that in considering the motion of a body when subjected to an earthquake, it was absolutely necessary to take into account the natural periodic time of vibration of that body; and they came to the conclusion that, in two widely different cases, the motion of a body relatively to the earth represented, with a very considerable amount of accuracy, the motion of the earth itself. In one case the body was a very quickly vibrating one, and its motions with regard to the box in which it was placed had to be very much magnified. In the other case the body was a very slowly vibrating one—slow compared with the motion of the earthquake, but its vibrations had to be damped by means of fluid friction. The second case was largely taken up and adopted in the making of seismographs, and all seismographs in use in Japan at the present time were based upon that second part of what they had called a "neglected" principle. They were bodies suspended in such a way that their natural periods of vibration were exceedingly slow. They still thought that the quickly vibrating body would give the best results, and they did not think that sufficient attention had been paid to the fact, that the friction which stilled the vibrations of the recording body ought to be

Professor Perry.

fluid friction. Having found a body which in its motion resembled the motion of the earth, they pointed out that it was necessary to register the motion of that body on three moving strips, one for the east and west motion, one for the north and south motion, and one for the up and down motion. In fact they pointed out how necessary it was to ascertain the actual motion of a point of the earth. Until that time observations had been made of the motions of such bodies as pendulums, hanging lamps, and hanging pictures—bodies which could get up a very considerable swing. The result was that sometimes a lamp, for example, would get up a great swing in a small earthquake, and sometimes after what was called a violent earthquake it did not seem to have any swing whatever. As another example, he might mention Mr. Mallet's seismometer, which consisted of a number of little vertical columns which rested on a level table. They were of different sizes, and of course the smaller column tumbled over with a small earthquake, and all the columns tumbled over with a big earthquake; and the direction in which they were found prostrate afterwards gave an indication of the direction of the earthquake. But it was found in Japan that the columns used to tumble in all directions; and on one occasion after Mr. Milne had, with the greatest possible patience, managed to balance a very small column, an earthquake which followed shortly afterwards upset every other column except this thin one. In the Papers to which he had referred they had calculated the periods of natural vibration of various structures, and had pointed out that if every part of the structure had a natural period of vibration very much quicker than the earthquake-vibration, there seemed to be no chance of its failure; but if a part of the structure had a period of vibration comparable with that of the earthquake, then differences in period of different parts of the structure might give rise to very great damage, unless there was a viscous resistance to motion which stilled the vibration. They pointed out that Japanese temples and houses which had very heavy roofs were not fastened to the ground, and were considered safe, and no doubt they were safe, but they rested on rounded detached stones, and if a slowly vibrating building of that kind were really well fastened to the ground the chances were, in fact it was almost certain, that it would be injured in the event of an ordinary Japanese earthquake. They also pointed out (in opposition to the practice of a considerable number of architects and engineers in Japan) that it was not right to have on a chimney a very heavy top, or to put a very heavy roof upon a building. They also demonstrated the danger of attaching

Professor Perry. chimneys to buildings, or attaching bodies together which had different rates of vibration. He mentioned those facts because the conclusions which they had drawn had been so thoroughly borne out by Mr. Milne's Paper. The obvious joke at the time was that the principle which they had called a neglected principle would remain neglected still. Indeed, when he considered the unscientific way in which observations on earthquakes were made at the time, and the exceedingly large amount of work involved in a scientific investigation of earthquakes such as they had proposed, his impression was that it would remain a neglected principle. But Mr. Milne's Paper had completely removed that impression from his mind. When the Author began his work on earthquakes in Japan, Professor Perry feared that he was undertaking a hopeless task. The difficulties of meteorology were great; but they seemed to him small in comparison with the difficulties attending the pursuit of such observations as Mr. Milne proposed. He was warned that it was a rather thankless pursuit, but he refused to consider that suggestion. He used every instrument old or new that he could lay his hands upon, and invented new instruments for himself. He buried microphones; he used long pendulums and Mallet's toppling columns. The slowly vibrating seismographs invented by Chaplin, Ewing, Gray and himself, had, in his hands, for five or six years recorded every earthquake that had taken place at Tôkiô, at many stations both on the earth and under the earth. He had had, in fact, a monopoly of the subject, and appeared actually to love earthquakes while others feared them. It was difficult to express his admiration for the patience with which the Author had adhered to such laborious scientific work. He regarded the Paper as the best that had yet appeared on seismology. The fact that it was possible to make a seismic survey; that in a mine or at the bottom of a 10-feet pit there was very little motion; that the nature of earthquake-motion was now known; that Stevenson's aseismic joint, after being discarded as useless, really possessed the elements of a very useful invention; and that it might now be hoped by experimental trench-digging or free foundations to ward off danger to a building; these appeared to him to be great additions by one worker to the knowledge of a very obscure subject in the course of six or seven years. Finally, Professor Perry referred to a pamphlet by himself and Professor W. E. Ayrton, "On Structures in an Earthquake Country," published in 1878.

[Mr. Brunton. Mr. R. HENRY BRUNTON, as the Resident Engineer, entrusted with the construction of the Japanese Lighthouses mentioned in

Mr. Brunton.

the Paper, congratulated the Author on being able to accomplish what he during the ten years he was in Japan had done his best to carry out. He had urged the Japanese Government officials to establish some system of observing earthquakes, but failed to obtain their sanction to move in the matter. In order to obtain the best possible information on the subject he put himself in communication with Professor Palmieri, of Naples, and the system he proposed to adopt was as follows:—At all the lighthouses round the coast records of meteorological observations were kept, which were sent to the lighthouse-office in the usual way, by the steamer calling once a month. By placing one or more seismographs at or in the vicinity of some of the lighthouse stations, records would also be obtained of the movements of the earth in different parts of the country. The adoption of this scheme, which had the approval of Professor Palmieri, afforded a means of judging of the positions of the centres from which the disturbances arose. The records obtained by Mr. Milne had been from observations made in such a narrow area that they were not of extended value. Assuming that an earthquake was brought about by some subterranean explosion, the disturbance caused by this was naturally transmitted to the surface of the earth, in its greatest intensity, in the line of least resistance, which was a line direct from the centre of the earth, called by Mr. Mallet, in his book on the Neapolitan earthquake, the "Seismic vertical." From the seismic vertical the disturbance diminished in concentric waves on the surface. Of course the direction and force of those waves were altered by the different geological formations through which they passed. He always supposed that rock or compact soil would continue the motion well, while marsh land or clay, by its great elasticity, would tend to absorb it. If that theory of earthquakes was an accurate one, the importance of discovering the position of the centre of disturbance was plain, because not only would this enable an estimate to be made of the probable severity of a shock at any particular place, but also of the character of the shock—whether the motion was likely to be a horizontal or vertical one. It was clear that the nearer the seismic vertical the more the motion would partake of a vertical character, and the farther from the seismic vertical, the more likely there would be no vertical motion, but simply a horizontal one. The statement of the Author, therefore, that the vertical component in his observations was so small that it might be neglected, only tended to show that at Yedo or Tôkiô, where his observations were made, he was at a considerable distance from the centre of disturbance. The Japanese, of whom he had made

Mr. Brunton. inquiries, had told him, unanimously, that the most destructive earthquakes were always those in which there was a distinctly appreciable vertical motion. Their destructiveness was in great measure to be accounted for by the fact that, the motion having been vertical, the localities were in close proximity to the centre of disturbance. During the discussion on his Paper on the Japanese Lighthouses, read before the Institution in 1876, Mr. Lloyd had given an instance of a globe on an ordinary table-lamp being pitched off by an earthquake without touching the chimney which protruded through it.[1] Likewise Mr. Brunton described how in a lighthouse at Sagami, situated about 20 miles from Yedo, the twenty-one Argand lamp chimneys were by an earthquake all pitched off their holders. The same earthquake turned round the upper six or seven courses of the chimney of the light-keeper's house, a column about 10 feet high, and 2 by 3 feet square, so that the upper part stood diagonally on the lower. These instances all give distinct evidence of a violent upward motion; they also pointed to the importance of a more extended system of observations. The Author had quoted a passage from Mr. Mallet's introduction to Palmieri's "Eruption of Vesuvius in 1872" in reference to the Japanese lighthouses. "Those edifices have been constructed so that they are presumedly proof against the most violent shocks likely to visit Japan; not, perhaps, upon the best possible plan, but upon such as is truly based upon the principles I have developed;" and he added, that this probably referred to the aseismic Tables which were devised by Messrs. Stevenson. To that Mr. Brunton emphatically demurred. Mr. Mallet had not, he believed, developed any system of aseismic joints, and there was no such joint in the constructive portion of any of the Japanese lighthouses. Mr. Mallet must have referred to the lighthouses as they existed. Two iron lighthouses were made in England, and sent out to Japan with those joints fitted to them, but they were (fortunately he thought) lost at sea. He said fortunately, because it gave him the opportunity of inquiring into the matter, and getting some personal experience of what an earthquake shock was. The decided conclusion to which he had arrived was that the tables of Messrs. Stevenson (from whom he was pained to differ) in their then existing state were impracticable and useless. Mr. Milne had stated the reason of their impracticability, which was their liability to be affected by ordinary disturbances, probably a great deal more than they would by moderate earthquakes. And the reason for their uselessness was, he believed, that every earthquake likely to be destructive to a well-built construction had a vertical motion. Those tables afforded no provision against this, but, by destroying the inertia of the structure, they would be probably mischievous in their effect. The decision at which he arrived in his designs for the Japanese lighthouses was to follow the principles enunciated by Mr. Mallet, and Professor Palmieri, to give the buildings weight and great inertia, coupled with a good bond between their various parts. Professor Palmieri stated that, although solidity and strength in a building did not afford perfect protection, still, so long as fracture did not occur, overthrow was impossible. That indicated the principle followed by him in the construction of the Japanese lighthouses. He thought that probably the improvement in the seismic joint made by Mr. Milne with shot, if he could get it sufficiently rigid as not to be moved by ordinary disturbances, might prove of some value in certain districts far removed from the centre of disturbance. In conclusion, he wished to express his sense of the extreme importance of such investigations to residents in earthquake countries, and to repeat that he was disappointed that the Author had not been enabled to carry his investigations over a wider field. Mr. Brunton.

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[1] Minutes of Proceedings Inst. C.E., vol. xlvii., p. 31.

Mr. Beaumon

Mr. W. W. Beaumont said he had not been in any country that deserved to be known as an earthquake country, but he had devoted a great deal of attention to the subject, having been engaged with the late Mr. Mallet in his investigations. Judging from the remarks of Professor Perry, he thought that he could not have read more than a fraction of the enormous quantity that Mr. Mallet had written on the subject of earthquakes.[1] Mr. Beaumont did not pretend to have read all that had been written by Mr. Mallet, but having been with him a good deal he had necessarily ascertained his views better than he could have done by reading. It appeared evident from the remarks of Professor Perry and Mr. Brunton, and from the Paper, that the origin of the Japanese earthquakes was, as a rule, at a very considerable distance from Japan. All the movements appeared to be horizontal, or so nearly horizontal that hardly any vertical component was observable. That was shown very plainly by the Author's observation as to the difference of movement at the bottom of the pit only 10 feet in depth. But it was not a new discovery. Mr.

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[1] See especially Four Reports to the British Association for the Advancement of Science. First Report, 1850, pp. 1–87. Second Report, 1851, pp. 272–320; Third Report, 1852, pp. 1–176; 1853, pp. 117–212; 1854, pp. 1–326. Fourth Report, 1858, pp. 1–136.

r. Beaumont. Mallet had observed that an earthquake was often cut off by a river, and a bank of a river had been seen in India on many occasions to lose a certain portion of its force, which was suddenly projected into the water. It had been found that an earthquake which had been noticed on one side of the river had not been observed on the other side, the energy of wave-transmission being dissipated by the projection of the material of the bank, and conveyed to the air and water. Such a wave of elastic compression could only be the expiring evidence of very distant impulse, and therefore of horizontal transmission. Another proof was that the direction of movement over even a very small area in Japan was very various, and any coseismal far from circular, showing that the impulse was probably distant, the direction of wave-emergence practically horizontal, and the velocity of wave-particle so small as to be affected by the heterogeneity of a very small depth below the surface. Professor Perry had referred to the columns used to indicate the direction and, to some extent, the intensity of the shock. Mr. Mallet only used that method where more delicate and better instruments were not obtainable. He had stated, in the directions published in "The Admiralty Manual of Scientific Enquiry," that these could be used when seismometers could not be obtained; but he would never have thought of employing them, nor of interpreting the indications of a few pillars, as giving information generally applicable from an area of observation so small as that described by the Author, and affording instrumental information of so complicated, confused, and of so obviously local a character as that from this part of Japan. Mr. Brunton's remark as to the measurement of earthquakes appeared to indicate that he would like to undertake a survey even very much larger than that suggested by Professor Perry; and the end of which would be that if he obtained the information suggested, he would have to make every structure capable of withstanding the vertical movements and vibrations of steep emergence, as well as those movements which were worse than vertical, namely those observable at what Mr. Mallet had called the meizo-seismal circle, the co-seismal boundary of a seismic area, and one on which the amount of destruction was the greatest. That was to say, within this meizo-seismal zone the direction of shock would be more vertical, and the overturning power less, while outside it the direction would be more horizontal, but with velocity of wave-particle reduced by distance of transmission. The aseismic joint was regarded by Mr. Mallet as merely a compromise. Even if it acted well, it would obviously only save

Mr. Beaumont

from destruction just the upper part of a lighthouse tower, the lower part still having to sustain the shock. If it were strong enough it would move with the earthquake, or would resist the movement. Mr. Mallet held the view that three different systems of building were more or less suitable in any particular country. First, the structures should be so strong that their own inertia would not destroy them. Secondly, when it was impossible to construct buildings strong in that way, by binding them together thoroughly, or, if of other materials, by making them like a monolith, then he would build them of lighter materials—wood, or a light iron framework—materials which had a greater amount of strength as compared with their inertia of rest or of motion. The third method would be one which would allow the structure to move a little. His idea was that a structure mounted on large rollers or large shot would be likely to move so freely, as to tend to its own damage or destruction, more especially in the secondary phase of the shock, and that the part so insulated would be pitched off or be more damaged than if it were ordinarily fixed. His view was that although not fixed, it should move only with difficulty. The Author remarked that Mr. Mallet had assumed that the action would always be of the nature of a sudden blow. It was difficult to understand how he could have come to that conclusion, for Mr. Mallet had taken the greatest trouble to show that the effect was not necessarily that of a blow, but might be one of extremely slow action. He had found that under favourable conditions, in an ordinarily heterogeneous rocky surface, as much as seven-eighths of the velocity of wave-translation was lost, and that the mischief was done by the often very low velocity of wave-particle. He had made experiments in Ireland over large areas and with heavy shots. In some cases he had produced imitations of earthquake waves, by large quantities of powder, and during the construction of the Holyhead harbour he had measured the impulses obtained by the explosion of from 6,000 to 12,000 lbs. of powder at a time.[1] He had obtained results, which were in several respects similar to some of those indicated by the Japanese observations, as to the tremors which preceded and succeeded the main shock. They were referred to at length in the first, second, and fourth Reports to the British Association for the Advancement of Science. Briefly, in most cases there were

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[1] Report of the British Association for the Advancement of Science, 1861, p. 201; and Philosophical Transactions of the Royal Society of London, vol. 152, p. 663.

Mr. Beaumont. secondary elastic waves accompanying the main earth wave. The subject appeared to resolve itself into what might be termed an intelligent comprehension of the effects of the inertia of structures. The reference to the chimneys, to the twisting of buildings, to the effect of a band binding one portion of a structure to another, and so on, tended to show that any structure should be built with reference to the strength of the material as well as its mass. The buildings could be resolved chiefly into two classes: those that were made to cope with constant shocks of small intensity, such as occurred in Japan, and due generally to a distant impulse almost wholly horizontally transmitted; and structures made to cope with heavier shocks due to a less distant impulse, and not so small in vertical amplitude of wave-particle or so small in velocity.

It appeared from observations extending over a lengthened period, apart from the Author's experiments, that in Japan, as a rule, the shocks though constant were not frequently of very great intensity; so that he thought it would be wrong to arrive at any general seismological conclusions from the observations described by Mr. Milne. His Paper was of more interest to engineers than some Papers which had preceded it, because it dealt with the question as to what should be done to prevent the destructive action of earthquakes, instead of dealing with them from a purely seismic or geological point of view. He would remark that seismology, which owed its existence as a science to Mallet, did not depend for its completeness upon the minute accuracy of any one construction of seismograph or seismometer. Nothing had yet been done which added to the completeness of the discoveries and enunciations made by Mallet, and although more elaborate instruments might secure valuable observational statistics, they must always be interpreted by reference to the laws discovered by him, and to his work on the subject.

Mr. Galloway. Mr. W. Galloway remarked that the first thing that had struck him in connection with the Paper, was the difference in the amplitude of the vibrations at the bottom and at the top of a pit 10 feet deep, a point that he regarded as new in connection with earthquakes. The general impression had been that a shock given to the crust of the earth would pass along the solid rock quite as well as along the surface deposits; and it had occurred to him that there must be something imperfect in the construction of the seismograph. Professor Perry had since described it, and it was obviously not so perfect as could be wished. He should like to see a series of results obtained simultaneously at different depths in a shaft at least 200 or 300 feet deep, before accepting such a conclusion as proved. The method of marking time and connecting the seismographs together, adopted by the Author, seemed perfect; the seismographs, on the other hand, were capable of much improvement. Some remarks had been made as to the observations proposed to be taken in the workings of collieries, with the view of foretelling the occurrence of outbursts of fire-damp. The proposal was made in this country about six months ago, and a good deal was then said respecting it. A Paper on the subject had been read before the North of England Institute of Mining Engineers, by Mr. Walton Brown,[1] who suggested that if seismological observations were taken in mines, it might be possible to foretell outbursts of fire-damp, and thus prevent the occurrence of explosions. Mr. Galloway had written a short article on the subject in "Nature," in which he pointed out that those observations could not possibly be of use as a means of foretelling, because if cracks were formed by the disturbance it would be impossible to say where they were and how soon the workings of a mine would strike into them; they might be struck the day following the earthquake or not for years afterwards. He was, however, glad to observe from the Paper that the vibrations underground were so much smaller than on the surface; it prevented, he thought, a complication from being introduced into the question of explosions in mines. Mr. Galloway.

Sir Robert Rawlinson. Sir ROBERT RAWLINSON, C.B., did not think that modern earthquakes could be clearly studied without some connection with geology, as there was no period in the history of the earth's crust, in which earthquakes had not occurred similar to those prevailing at the present time. Geology showed, on examining a good section of the earth's crust, that the prime dislocation and motion had necessarily been vertical, because mines were found displaced on the opposite sides of lines of faults; and on examining sand-stone rocks especially, it was found that the motion had been many times repeated, that the rocky sides had been rubbed together, until one had moulded itself into the other, and the substance of the rock had been altered and hardened in its character. When it was concluded that earthquake shocks were mainly horizontal, he ventured to say that every horizontal motion from an earthquake shock had proceeded from a vertical motion; in other words, that the line of axis which had caused the motion had been vertical, and that there had been a settlement of a portion of the earth's crust upon itself, causing reaction. It had been observed in

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[1] Transactions, vol. xxxiii., 1883–84, p. 179.

Sir Robert Rawlinson. California that the earthquakes in some regions had a reacting or dancing motion up and down for a considerable period of time. That was what must have taken place from remote periods, when the surfaces of the rocks along lines of faults had been rubbed together. The horizontal motion observed was an effect thrown out from the vertical motion, and carried on to the extent to which the shock was capable of being projected through the upper portion of the crust of the earth. The broad general fact had been laid down by Humboldt, that in all parts of the earth's crust liable to earthquake shocks there was, as shown by pendulum experiments, the thickest and densest crust—that there was over such areas a super-incumbent mass of newer material laid upon the older rocks, such as the granites; and that in those places where the earth's crust had accumulated to a great extent of thickness, the extra weight of the earth tended to sink upon itself, or upon what had been termed "central heat," which, if by this was meant that the internal mass of the earth was semi-fluid through heat down to the centre, he did not believe. He thought that the earth's crust might be compared to a shell which was solid to a certain extent, and that the inner portion, whatever might be the thickness of the shell, would be cool, not probably as the outer surface, but as solid; and that the line of central heat would be in the centre of that solid crust, produced by the cohesion of the material, the heat being caused by the pressure of gravity. He had never tested a bar of good iron to the breaking point, without finding that when it was examined between light and dark, heat was developed visibly to the eye, and always electrically. In the newer and denser portions of the earth, there was a wasting of the great mountains, and the detritus was carried down by the rain and rivers and spread out over the profound depths of the ocean, which would naturally be the thinnest crust for the time being. When that accumulation and added pressure had taken place to a sufficient extent earthquake shocks were set up, and there were vertical risings of the earth's crust; while in the older worn-out portions which had become thinner and lighter there was diminished heat, which tended to subsidence. All the regions of active volcanoes had the densest crust. The loftiest mountain ranges were, in the earth's history, the youngest, far younger than the mountains of Scotland. That however was an abstruse question, and he would say no more about it.[1] With reference to the structures to be erected in earthquake countries, the remarks of the Author were not only applicable to those countries, but also to our own. Sir R. Rawlinson had paid attention to the construction of tall chimneys, and he knew that some in England had been brought to ruin by loading them with great lumbering and projecting cornices. Many buildings in London would not be safe if an earthquake shock took place, loaded as they were with overhanging stone cornices that did not balance themselves upon the structure, but were tied by iron clamps behind. That form of building ought not to be permitted. He agreed with the Author that structures that had to rise in elevation should be lightened as they rose, having a good foundation deep in the ground. Sir Robert Rawlinson.

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[1] This central heat theory was promulgated about 1842, in a Paper, published in Liverpool, explaining that pressure generated heat and reactions.

Mr. W. H. THELWALL said that the earthquakes to which the Author had referred appeared to be of a rather mild character. The earthquake that occurred in the Island of Ischia in 1883 was of a most destructive character, and an enormous amount of damage was done in the island, two thousand persons having lost their lives and many more being injured. A Commission was sent into the island by the Italian Government to obtain information about the earthquake, and to frame rules for the rebuilding of the structures. One town was so much injured that only one house out of more than six hundred was not damaged. It was suggested at the time that this was not an ordinary earthquake, but was the result of the falling in of subterranean caverns, but the Commission were convinced that this was not the case. The Commission first examined the island with the view of ascertaining the locality of the centre of the shock, and what portions of the island were comparatively free from danger, so as to recommend certain sites upon which buildings might in future be erected, and to condemn others as dangerous. It was ascertained that, speaking generally, buildings founded upon hard solid lava had withstood the shock successfully, while those founded upon looser or lighter materials, such as tufa or clay, had suffered very much. The effect of the shock was comparatively slight upon the sea-shore, but sites for building should have sufficient elevation (say from 30 to 50 feet above the sea), to be out of the reach of any violent waves caused by the earthquake, as such waves frequently caused more damage in washing away buildings than was occasioned by land shocks. When the shock occurred there was a very violent upward movement, the movement not being confined as in Japan to a horizontal direction. In regard to the re-erection of buildings it was pointed out that the first thing to do was to select eligible sites and to build wherever Mr. Thelwall.

Mr. Thelwall. possible upon lava, and where that was not possible, to dig down to comparatively solid ground, and then fill in a heavy platform of masonry or concrete 3 or 4 feet thick, extending over the whole area of the building and projecting 3 or 4 feet beyond. The building of any kind of vaulting above ground was forbidden. Light arches were only to be allowed over windows and openings of that kind. The heavy flat roofs formerly used to a large extent were condemned. The Commission recommended that buildings should be chiefly constructed with an iron or wooden framework, carefully put together, joined by diagonal ties, horizontally and vertically, with spaces between the framework filled in with masonry of a light character. The joists and the roof-trusses were to be firmly connected together. In plan, buildings should be square, and where the direction of the last shock could be traced, one diagonal should be placed in this direction. Not more than two storeys above ground were to be allowed, and there might be one underground, but it must be of very moderate height. In no case was the height from the lowest point of the ground to the top of the walls to exceed 31 feet. Openings for doors and windows were to be vertically over each other, the jambs being not less than 5 feet from the corner of the building. No openings for flues were allowed in the thickness of the walls, and no projections from the face of a building, except light balconies of wood or iron. In solidly built structures, and particularly if there was only one storey above ground, the roofs might be covered with tiles, but these must be light and fastened with nails or hooks, so as not to be displaced even by violent shocks. In other cases zinc or corrugated iron was to be used, proper means being taken to keep out the heat. In certain buildings flat roofs would be allowed, but their construction must be of the lightest possible character.[1]

Correspondence.

Mr. Bush. Mr. G. Bush considered it difficult to lay down any rule as to the motion of earthquakes. In the parts of South America where he had resided, the houses, and all large buildings such as warehouses, were erected of wood-framing and walled in and roofed with galvanized iron. One Government block of buildings in Iquique, built of stone and cement, had withstood very severe shocks, especially one in 1877, when it resisted the great wave. Mr. Bush. Earthquakes did not interfere much with modern structures, but there was great danger of fires, which had been caused in several instances by the upsetting of kerosene lamps. The current of shocks in Iquique was always north-east and south-west. At the silver mines the motion was scarcely felt, but the rumbling noise was very audible. On the Pampa of Tamburugal, which was 3,500 feet above the sea, internal rumblings were heard almost nightly, but surface-motion was only occasionally experienced. The railway had not suffered from shocks, excepting in cuttings, where a few stones were occasionally discharged. Off the coast, on board steamers during an earthquake, the motion was distinctly felt. Since the Chilians had taken possession of Iquique, the Jefe Politico, Mr. Bulnes, had issued a decree that all roofs should in future be made of galvanized iron, to prevent sparks from setting fire to houses and other buildings; formerly they were constructed of wood and then covered with shells from the shore.

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[1] The report of the Commission was published in full in the "Giornale del Genio Civile" for 1883. Fourth Series. Vol. iii., p. 541.

Professor C. Clericetti, of Milan, remarked that the Author was Professor Clericetti. not aware of what the Italian Government had recently done in reference to earthquakes, and his statement on p. 290 should be modified. In July 1883 a terrible shock occurred in the island of Ischia, by which more than 2,300 persons lost their lives. Several descriptive memoirs had been written about it. The principal centre of destruction was Casamicciola, where the houses were entirely destroyed. On the 15th of September of the same year the Minister of Public Works appointed a Commission to consider the reconstruction of the public and private buildings which had been injured. The Commission was to indicate the most suitable positions in the island for building new thermal baths, and for the erection of a mortuary for the victims of the catastrophe. The report of the Commission was published in Rome at the end of 1883.[1] It treats at large of the different methods of construction proposed for localities threatened by earthquakes, and of those most suitable for the island of Ischia. Models of the constructions in wood and combinations of wood and masonry which were adopted had been exhibited at the Turin Exhibition.

Mr. Henry Dyer, Honorary Principal of the Imperial College Mr. Dyer. of Engineering, Tôkiô, wished to bear testimony to the great industry and ability displayed by the Author in his investigations

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[1] Relazione della Commissione per le prescrizioni edilizie dell' isola d' Ischia istituta dal Ministro dei Lavori Pubblici (Genala) dopo il terremoto del Luglio 1883. Inst. C.E. Tract, 4to, vol. 91.

Mr. Dyer. relating to Japanese earthquakes, a work which had earned for him an honourable position alongside Mallet and Palmieri, the founders of the science of seismology. The purely scientific aspects of the subject had been fully discussed by the Author in his reports to the British Association.[1] As he was either directly or indirectly interested in the chief buildings mentioned by the Author, he would briefly refer to the results of his experience in the design and the construction of buildings in Japan. He thought that the supposed necessity for making special designs for buildings in earthquake countries had a very bad effect on the development of architecture in those countries, as he was strongly of opinion that the main things to be attended to were good construction and a few elementary principles, which a short study of earthquake phenomena was sufficient to impress on any intelligent observer. From observation both in Japan and on the continent of Europe he could say that if more attention had been paid to construction and to those elementary principles, little or no damage would have been done by ordinary earthquakes. Against extraordinary shocks by which the earth was rent asunder it did not seem possible to take any precautions. Mallet held similar opinions, for in reference to the Neapolitan earthquake of 1857 he wrote:[2] "It was evident, that had the town generally been substantially and well built, or, rather, the materials scientifically put together, very few buildings would have been actually shaken down, even in those localities where the shocks were most violent, and their directions the most destructive. Thus the frightful loss of life and limb were as much to be attributed to the ignorance and imperfection displayed in the domestic architecture of the people, as to the unhappy natural condition of their country, as respects earthquakes;" and he gave many examples of buildings in which the masonry was of the best class, and such as would be so recognized in England, which stood uninjured in the midst of the ruins of those constructed of rubble stones with large ill-filled mortar joints. An ordinary Japanese house consisted of wooden uprights resting on rough round stones, and supporting a heavy roof. The uprights had few diagonal struts, and as the joints soon gave way, it was almost impossible to imagine a structure worse adapted for resisting earthquake shocks, as it was wanting in lateral stability, and the result was a total collapse with a comparatively slight shock. It ought to be observed that although the Japanese carpenters were very expert in the use of their tools, and the joints of their buildings were very neatly made, they had little or no idea of the proper disposition of material to ensure approximate uniformity of stress, and it was often found that the joints were snapped asunder on a sudden shock being given to them. The fire-proof stores of the Japanese were made of a framework of wood covered with mud plaster to a thickness ranging from 1 foot to 2 feet, and had a roof of the same materials covered with tiles. Being at once stable and fireproof, they generally withstood the effects of both earthquakes and fires. These structures formed the patterns from which many of the buildings used by the foreign residents were designed, and here again was an example of the results of unthinking copying. The stores served their purposes well, but the so-called foreign houses, built with wooden frames and covered with thin layers of bricks, stones, or tiles, instead of mud, were deficient both in strength and stability. The wood, generally being green when first used, soon rotted, the joints of the covering gave way, and after a few years it was found cheaper to build new houses than to keep the old ones in repair. Add to this the fact that they were for the most part heated by stoves with badly fitted pipes, or with fireplaces with chimneys loosely built, and of the design mentioned by the Author, loaded at the top with a heavy head, which vibrated with a different period from the house, and so in consequence were generally cut across at the point where they passed through the roof on the occurrence of a slight earthquake, and it would be understood that earthquakes were not the only cause of the numerous nervous disorders so common among the foreign residents in Japan. Mr. Dyer.

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[1] Reports of the British Association for the Advancement of Science, 1881, p. 200; 1882, p. 205; 1883, p. 211; 1884, p. 241.

[2] The "Great Neapolitan Earthquake of 1857," vol. i., p. 94.

The main building of the Imperial College of Engineering at Tôkiô had been designed and built under the impression that fire was more to be dreaded in Japan than earthquakes, and therefore it was constructed almost entirely of brick, and very few special precautions had been taken against earthquakes. Some parts of the roofs were supported loosely on the walls, and at first the bricks were bonded with hoop-iron, but ultimately these precautions were abandoned, and great attention was paid to the construction, to ensure that the mortar and bricks were good, so that the walls might be as strong through the joints as through the bricks; and, as far as possible, a uniform distribution of weight was attempted, so that the different parts might vibrate with the same period. Where that was not possible it was so

Mr. Dyer. arranged that a difference in period could not do much harm. The height of the walls was moderate, and the towers were strengthened by tie-rods at the different stories. Chimneys were almost entirely avoided, the buildings being heated by steam. The cracks mentioned by the Author were not caused by the earthquakes, but by the foundations not being sufficient, a result which was anticipated, and which arose from an undue wish to economise. No doubt after those cracks had been started the earthquakes caused them to lengthen, but not to any serious extent.

The workshops at Akabane, which were connected with the college, gave greater difficulty than the college buildings. That part of Tôkiô was an old sea bottom, and below the surface soil a layer of mud about 30 feet deep existed. Piles were driven into this and bound together, and on the top was placed a bed of concrete on which the workshops were built, so that they were really floated on the mud, and the result was perfectly satisfactory, no cracks of any importance appearing.

The stones most commonly used in the foreign buildings in Japan were not well suited to withstand the shocks of earthquakes, being for the most part volcanic mud stones. There was an abundant supply of granite and the harder volcanic rocks, but as a rule they were expensive on account of the difficulty of transportation. If these could not be used it would be better to build entirely of brick, for the soft stones weakened the buildings. The walls of the moats and castle gates in Japan, which had all the appearance of great stability, were almost always in a state of unstable equilibrium. The large stones were generally square or polygonal on their faces, but pyramidal in shape, coming very nearly to points at their backs, the spaces being filled up with loose rubble without cement of any kind. The bearing-surface at the faces of the stones being small, a slight movement was sufficient to bring the whole construction down, a result which, however, was no measure of the intensity of the earthquake, as the building might have previously been on the point of slipping.

For dwelling-houses the modifications of external design required in Japan, as compared with Great Britain, arose not so much on account of the earthquakes as from the heats of summer, the colds of winter, and the typhoons of autumn. Iron roofs were good from a merely structural point of view, but in summer it would be almost impossible to live in the houses provided with them. If a non-heat-conducting material of the same strength and durability as iron could be found, it might be used. Similarly iron chimneys would be the causes of numerous fires, even when two tubes were placed concentrically with an air-space between them, for careless workmanship, bad material, and corrosion would soon render them dangerous. If the houses were so designed as to be comfortable as regarded temperature, and the construction made in good brick, or equally strong stone and mortar, so that the walls were of nearly a uniform strength, if no unnecessary top weights were used, and if the various parts did not vibrate with different periods, they would withstand all ordinary earthquakes, and other precautions would be unnecessary, as these generally produced results more serious than those due to the earthquakes. Mr. Dyer.

Mr. Earnshaw

Mr. D. Earnshaw, during a residence of twenty-five years in Manila had experienced hundreds of shocks of earthquakes, some of them very severe. The first was in June, 1863, which ruined more than half the buildings in the city including the cathedral, where many lives were lost, and the last in 1880 when three very severe shocks took place in two days, an unusual occurrence, as one heavy shock generally occurred at intervals of about eight years. He had come to the conclusion that it was best to build as strongly as possible, and in the following manner: The building should be two-storied, and depend for its stability entirely on wood, and not on stone or brickwork, which should only be used for foundations and filling in sides or walls; wooden pillars about 12 inches square of sound well-seasoned timber should be set in concrete foundations about 6 feet deep, according to the soil, and carried up to the roof. These should be well strutted or tied with timbers 9 inches square, so as to form one frame set level with the stone floor below; timbers for carrying the upper floor-rafters should be well fastened through pillars with strong iron bolts and nuts; and the extremities of the timbers for carrying the roofing should be supported by three wooden or wrought-iron knees, with strong hardwood cotters through the projecting ends. In fact, the whole structure should be tied and bolted together as in building a ship. Many materials had been used for roofing, such as the heavy tiles made in the country and others imported there, when in 1880 fully 60 per cent. of the buildings in Manila having been ruined, an order was issued by the municipal authorities to use corrugated-iron or zinc-sheeting for that purpose. A diversity of opinion existed as to which was the best and most suitable, for not only had earthquakes to be guarded against, but an intense heat and disastrous typhoons. With regard to the latter he had seen in 1881 sheets of

Mr. Earnshaw. iron flying about in the air like paper. On that occasion he had seen an iron roof carried through the air entire and landed on the top of a house some 200 feet distant. He therefore thought that a light, strong tile roofing was preferable to any other. It kept the houses much cooler than iron; it was not liable to be blown off, and it was much more durable than iron, and therefore cheaper in the end. The only objection to tiles was the extra weight, but if the ceiling under the roof was boarded, as it should be, with boards 1 inch thick, it would be strong enough to bear the weight of the roof should it fall in a severe earthquake. The house in which he lived covered an area of 5,000 square feet; it had a heavy tile roof which had withstood all typhoons and earthquakes, including the heavy one in 1863, and it was in thoroughly good condition. He considered arches over doorways useless, as they invariably gave way under a vertical movement, which as a rule caused much destruction; above doorways and openings timbers of sufficient strength should be inserted. Only in the bottom part of the building should stone or brickwork be used, and this should be tongued and grooved so as not to fall out of place, though any part might fall without materially affecting the strength of the building. The top storey and all partitions to rooms should be of boarding; other materials cracked or fell, and for a time rendered the house uninhabitable, besides damaging the furniture. Another most important thing to guard against in Manila was the "anay," or white ant, which should be done by carefully selecting the timber, as the majority of the buildings which fell in an earthquake were previously injured by white ants, causing them to give way with a moderate shake. Especial attention should also be paid to the quality of the lime and mortar used in construction. He could not agree with the Author as to the advisability of adopting loose foundations, as the impetus given to a structure by a heavy shock, would certainly cause the whole to move a great deal more than if the pillars were fixed in solid foundations, and firmly tied together. Neither did he think that one part was less liable to movement than another. The most solid ground should always be selected for building on, and close proximity to the banks of rivers and canals should be avoided.

Professor Forel. Professor F. A. Forel, not being an engineer, could not deal with the practical side of the question, but would confine himself to expressing his high sense of the value of the work carried out by the Seismological Society of Japan, and especially by the Author. The ardour with which scientists in that country had set to work to study seismic phenomena, and the ingenuity they had

Professor Forel.

displayed in devising instruments for recording them, were beyond praise. Professor Ewing's were the first seismographs to give an authentic diagram of earthquake shocks, and while in Europe investigators were embarrassed by delusions of long standing, it had remained for the younger science of the far East to discover the true character of these great natural disturbances. More had been learnt from the seismograph-tracer of the Anglo-Japanese observers in two years, than twenty centuries of European science had been able to show. For his own part, he was happy to be able to confirm the discoveries of his colleagues of the East. The thousands of individual observations on earth-tremors in Switzerland which he had collected since 1879, had not been made with specially designed instruments, and they recorded only subjective impressions or the objective observations of witnesses often ill-prepared for the very delicate study of these phenomena. Nevertheless they had indicated clearly the correctness of the interpretations of the Seismological Society of Japan. The orogenic earthquakes of Switzerland were similar in their symptoms to the earthquakes (of presumed volcanic origin) of Japan. He was also constrained to admit, from careful observation, that the shock was composed of a series of oscillations, relatively very slow, of small amplitude, following one another at a clearly defined interval; at first increasing in intensity, and then decreasing. The horizontal movement was of much greater intensity than the vertical, but the direction of oscillation varied much from one locality to another; often from one district to another of the same town; at times even from one house to another. Sometimes it was the longitudinal component that dominated; sometimes the transverse. He could confirm, also, the great difference in the sensitiveness of the soil to reaction under the influence of seismic disturbance. Certain parts of a town might suffer much more than others from the same shock. Feeble shocks were never felt in particular quarters; in others they were always felt. The labours of his colleagues in Japan had thrown a new light on his observations. Thanks to their seismograph-diagrams, he was the better able to understand the effects shown at home. He was of opinion that the Japanese observations were applicable to the Swiss earthquakes, and he was glad to have the opportunity of testifying to the great merit of the work of the Seismological Society of Japan, and of its founder, Professor Milne.

Mr. Holtham.

Mr. E. G. HOLTHAM remarked that the Paper furnished data for an estimate of the utility of observations entailing much labour and ingenuity. Most of the Author's conclusions were founded

Mr. Holtham. rather upon ordinary information and common sense, than upon disclosures afforded by his experiments or the records of instruments. The Tables given could not be said to establish more than the diversity that might characterise simultaneous indications, and the triviality of the circumstances to which this diversity was ascribable; and it followed that but little importance could be attached to such "seismic surveys" as the Author suggested. The line of averages at the foot of each Table was no doubt intended to facilitate comprehension of this point, though it must be observed that the number of observations varied greatly in different columns. The diagrams, showing amplitudes of vibration at various recording stations, should have been accompanied by some kind of scale, though the information contained in the Tables apparently implied that the diagram-amplitudes were about five times as great as the earth-movement, the instruments being so constructed as to enlarge in recording, to some definite proportion that might as well have been given. The Author's conclusions were likely to be of use rather to the domestic builder or architect than to the engineer, who as a rule did not require to be cautioned against placing excessive weights at unnecessary heights. The vibrations set up by wind-pressures in the case of lofty structures were probably of more importance, and demanded more studied provisions than any recorded earth-tremors. Ordinary engineering structures such as girder-bridges, arches of moderate span, station-sheds and roofs appeared proof against moderate shocks. None of the railway-works executed in Japan previously to 1882 had suffered from the effects of earthquakes, the greatest damage coming within his notice, as attributable to two rather severe shocks in 1880, being the starting of a few facing-stones attached to a timber-framed building of the kind generally considered in Japan to be earthquake-proof.

There was, however, an important class of structures upon which earthquake vibrations might have serious detrimental effect, namely, retaining walls. The Author rightly pointed out that any abrupt change of surface-conformation afforded scope to a free wave in place of a mere vibration. Nevertheless even in this case the dimensions usually given would suffice to protect a wall against overthrow; but if the toe of the wall were not effectually secured against displacement, there might be a forward movement. In some of the old castles of Japan, very lofty scarp walls had withstood earthquake-vibrations satisfactorily. This was probably owing to the great batter or slope given to the lower part of such walls, which united in a curved sweep with the nearly vertical upper portion, making the whole approach the form of a comparatively small terrace-wall, erected upon the top of a heavily-weighted or pitched bank possessing great stability. Mr. Holtham.

Mr. H. S. Ridings observed, from an experience over several years, when Resident Engineer of the Iquique Railway, Peru, that, as stated by the Author, the effect of earthquake vibrations varied much according to the nature of the ground. The confused direction of the motions was very apparent in structures embedded in sand, heavy bars of iron having been in May, 1877, twisted into an S-shape. The subject of free foundations was of great importance. Instead of shot, a much simpler, cheaper, and more effective plan would be to interpolate a layer of clean coarse sand 1 inch thick, in a convenient position, care being taken to prevent it from escaping laterally, and from being washed out. The wall above this layer should be inset 2 inches or so on both sides. These remarks referred to concrete, brick, and stone structures. One of the largest buildings in Iquique had a heavy framework of timber, well braced, the interspaces being filled in with brickwork; this had resisted heavy shocks of earthquake. Mr. Ridings.

Iron buildings and timber frame-houses, if carefully designed, and with an extra amount of diagonal bracing, seemed well suited for earthquake countries; but in the tropics they needed special provisions for ventilation. Such structures should not be bolted down to foundations of concrete or brickwork, except of course in the case of machinery, and one well-known engineer had found that it was better to embed iron columns in concrete, rather than to hold them down with bolts.

The value of careful bracing had been well shown in the case of a high timber staging and platform, carrying iron water-tanks, at Pozo d'Almonte, a station situated in the great plain of Tamarugal. Such a structure, from its top-heavy character, was particularly liable to failure during an earthquake. The great earthquake of May, 1877, was at this place so violent that the ground was cracked, and the station-master, fearful of being engulfed, stretched himself at full length between the rails along the sleepers, thinking that the rails would form a sort of girder-bridge over any chasm which might open. Notwithstanding this excessive vibration the tanks remained in their places and the staging was uninjured. Again, he had seen a well-framed timber house of one of the Iquique merchants, the day after the earthquake, floating about in the bay, with a list caused by a heavy piece of furniture, but perfectly intact. This house not only

Mr. Ridings. resisted the earthquake vibrations, but the shocks of the great earthquake sea-wave.

It did not seem that the violence of the shock always diminished rapidly with the increase of depth below the surface. In many parts of Peru the ground for some feet below the surface was harder than farther down, which was probably due to excessive solar action for ages in a rainless district. In one case an English miner was employed to sink a shaft in a valley near La Noria. He pierced through the upper crust and got down to a depth of about 100 feet. One Saturday a payment on account was sent up to him from Iquique. This he placed in a box in the tent which he and two native helpers occupied, and then was let down by the natives by an ordinary jack roll and bucket. When down, the men pulled up the bucket, and decamped with the money. The miner tried to escape by cutting holes for his hands and feet, but a strong shock of earthquake occurred, and he fell from a height of about 15 feet to the bottom, from whence he was subsequently rescued.

As regarded vertical motion, the engineer's camp among the mountains must have been at one time pitched in the vicinity of an epicentrum. Once, on being aroused from sleep by an earthquake, he observed that the tent-pole was moving violently up and down. It should, however, be remarked that the relative heights of bench-marks did not appear to be altered by earthquakes. The City of Arequipa, Peru, was particularly liable to earthquakes owing to its proximity to the great volcano, the Misti, 19,000 feet in height above sea-level, the city being 7,000 feet above sea-level. The general construction of the houses was peculiar. A light-coloured volcanic stone was largely used; this, when lately quarried, was easily shaped, and it hardened gradually. The roofs were for the most part strong arches, a very good mortar being used. In the earthquake of 1868, it was not so much these arches which failed as the walls, and the spandrels between the arches at front and rear. In some parts of the city arches extending in one direction stood, while those at right angles to these were thrown down. Since 1868 a good many corrugated-iron roofs had been introduced, but they were not well suited to the climate, and were not durable.

In conclusion, a short extract from the Paper on the Iquique Railway, published in 1883, might be interesting.[1] The earthquake of the 9th of May, 1877, occurring as it did at night, and

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[1] Minutes of Proceedings Inst. C.E., vol. lxxii., p. 189.

Mr. Ridings. without warning, involved a direct outlay (for materials and wages) of $17,000 on the Iquique section, and a rather smaller, but still considerable expense on the Pisagua section; and the loss from suspension of traffic during repairs was of course much more serious. On that occasion the town line, used for taking the nitrate of soda to the various stores and wharves along the sea-shore, was destroyed, and in some places completely obliterated; the railway pier also was washed away, and the water-condensers were much injured. Up the mountain the rails were in many places twisted in a most extraordinary manner; embankments were shaken down, leaving the rails suspended in the air, and rocks were thrown into the cuttings.

Mr. Stevenson. Mr. D. A. STEVENSON remarked that the Author appeared to claim as his own the principle of resting structures on balls, so as to break the continuity of the building with the foundation, with the view of mitigating the effect of earthquake shocks on the superincumbent structure. Mr. Stevenson and Professor Piazzi Smyth, the Astronomer Royal for Scotland, had had some correspondence on the subject with the Author,[1] in which they pointed out that Mr. David Stevenson had proposed this method in a Paper entitled, "Notice of Aseismatic arrangements, adapted to Structures in Countries subject to Earthquake Shocks;"[2] and further, that Messrs. Stevenson had designed and sent out to Japan, in 1869, several tables fitted with this contrivance for carrying the delicate optical apparatus in the lighthouses of that country.[3] He was pleased to find that the Author's experience proved the efficacy of Mr. Stevenson's method, more especially as he thought the lighthouse tables sent out to Japan had not had a proper trial. Thus Mr. Simpkin, who was employed in the lighthouse service in Japan, and who returned home in 1884, informed him that at Isuragisaki and Kashmasaki lighthouses the aseismatic tables were firmly strutted with timber to prevent any motion, as inconvenience was felt from the oscillations of the table, when winding up the machine and cleaning the apparatus; the steadying screws sent out with the apparatus (made so that any degree of rigidity might be given to the table) having not been put in at these stations. These two were the only lighthouses at which any damage had been done by earthquake, while those stations at which the tables were in operation had never suffered at all, although they had been

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[1] "Nature," vol. xxxii., pp. 213 and 573; vol. xxxiii., p. 7.

[2] Transactions of the Royal Scottish Society of Arts, 1868, vol. vii., p. 557.

[3] Minutes of Proceedings Inst. C.E., vol. xlvii., p. 27.

Mr. Stevenson. repeatedly subjected to shocks. Mr. Brunton had indeed expressed the opinion, and adopted as a principle, that "solidity, weight, and strength, as opposed to lightness and flexibility," was best, and he was by no means alone in this opinion as to the unsuitability of the aseismatic arrangement, for Mr. Robert Mallet, when consulted by Mr. Stevenson as to the exact phases of an earthquake shock, had written in 1868, acknowledging the receipt of a description of Mr. Stevenson's proposal, that if the balls and plates proposed were confined to the apparatus in the lightroom, "he would augur much more favourably of the result being satisfactory"; but his own notion for Japan, or other shaky places, would be to make all the towers rather of timber or of boiler-plate. In the designs for the aseismatic joints for the lighthouse towers, which were lost at sea, the balls varied in number and size with the height of the towers.

SECT. II.—OTHER SELECTED PAPERS.

(*Paper No. 1995.*)

"On the Energy of Fuel in Locomotive Engines."

By GRANVILLE CARLYLE CUNINGHAM, M. Inst. C.E.

THE object of this Paper and of the accompanying Table, is to show, by data obtained from different railway companies, what is the amount of fuel consumed per unit of work done by locomotive engines; how this consumption varies on different lines of railways; and how the energy of the fuel utilized compares with the full energy, in other words, how much of the energy is used, and how much lost.

The consumption of fuel per unit of work, that is, per ton weight moved 1 mile, is perhaps the most certain and reliable scale by which the capacity of a railway for doing work can be measured, and compared on the same scale with another railway. Any estimate based upon cost is misleading, since the price of labour, fuel, and everything that enters into the working of a railway, varies at different times and in different places. It might thus happen that a line showing a large cost per train-mile, or per car-mile, was more economically and carefully worked, and better able to do the work for which it was constructed, than another showing a smaller cost per car-mile. The comparison plainly depends upon the cost of labour and material in the two localities, and is vitiated by the rise and fall of markets. No true comparison of the respective railways, or even of different periods of the same railway, can be made until such vitiating elements have been eliminated, and a basis arrived at which shall be common to each, and unaffected by any adventitious circumstances. In the consumption of fuel per unit of work there exists such a common basis of comparison, and one which demonstrates the capacity for doing work which the railway possesses. For the consumption of fuel is almost an absolute standard, varying only with the quality of the fuel used, and is not affected by any other uncontrollable circumstance. Thus, if on one line of railway the consumption per ton moved 1 mile is very much greater than on another, it is evident that on the former the gradients and curves, and such elements of resistance, must be more severe than on the latter; and that therefore the latter line is the better able to do its work, and can, other things being equal, do it more cheaply. Of course other circumstances may cause an unusual consumption of fuel, such as severity of climate, inducing large evaporation and loss of heat; or badly-designed engines, resulting in waste of fuel. But even these are matters that can be controlled, because the first may be obviated by having the engine more thoroughly protected from the weather, and the second by improvements in the type of engine. With similar engines acting under not very dissimilar climatic influences, it remains that the consumption of fuel per unit of work may be taken as a certain index to the character of the railway.

In preparing the Table which accompanies this Paper, considerable difficulty has been experienced in arriving at the requisite data. The published annual reports seldom give the information in the direct manner in which it is required; but all the figures made use of have been drawn either from the published reports, or from information obtained directly from the railway officials.

On the Canada Southern Railway, where the consumption of fuel is lower than on any of the other lines, the gradients and curvature are very light. The main line of this railway extends through the southern part of the province of Ontario in Canada, from Fort Erie on the Niagara River, where the International Bridge gives access to the State of New York, and opposite to the city of Buffalo, to Amherstburg on the Detroit river, separating Ontario from the State of Michigan. The Detroit river is crossed by ferry-boats, on which the carriages are taken over to Grosse Isle; from whence they run into Toledo (where connection is made with the Wabash railway system), or into Detroit (where connection is made with the Michigan Central system), over the Toledo, Canada Southern and Detroit Railway. The distance from Fort Erie to Amherstburg is 229 miles, and throughout there is no gradient steeper than 15 feet to the mile (1 in 352), and the alignment is remarkably free from curves. On the western portion of the line, the distance from St. Clair junction to Amherstburg, 107 miles, is made up of two straight lengths of 53 and 54 miles, joined by a light curve. The same gradient is maintained on the Toledo, Canada Southern and Detroit Railway, and on the St. Clair branch of the main line. The only parts of the system on which steep gradients exist are the Erie and Niagara, and Michigan Midland lines; but on these the traffic is extremely small, and they aggregate only 45·28 miles in extent, as compared with 403·64 miles of the entire system. The locomotives used are of the Baldwin type, with two pairs of driving-wheels coupled, and weighing about 60,000 lbs. on the drivers.

The main line of the Michigan Central Railroad, which extends through the southern part of the State of Michigan from Detroit to Chicago, is 284·07 miles in length, but with branches and leased lines, it comprised 949·59 miles in 1881. The gradients on the main line and branches are considerably steeper than those on the Canada Southern, and in places reach 52 feet to the mile (1 in 100). The locomotives used are similar to those on the Canada Southern, and the fuel is also similar, being bituminous coal from Ohio.

The Lake Shore and Michigan Southern Railway extends along the southern shore of Lake Erie from Buffalo to Chicago, with branches to Detroit and other places. The total mileage of the system in 1880, including leased lines, was 1,177·67, and of this the length of main lines is 504·49 miles. The gradients of the main line are considerably easier than those of the Michigan Central, and nearly as good as those of the Canada Southern Railway. The engines and fuel are similar to those on the lines before-mentioned.

The Hannibal and St. Joseph Railroad is in the State of Missouri. Its mileage in 1880 was 292·35. From the length of trains hauled, the gradients would seem to be steep.

In preparing the Table in the Appendix, information has not always been obtainable from the printed reports in the exact form required. In these cases the method adopted for supplying the particulars has been as follows. The total amount of coal and wood (the latter turned into its equivalent in coal) consumed is noted. When the amount to be apportioned of the freight and passenger services respectively is not stated in the printed report, the total amount is divided into two portions in the ratio of the respective engines, mileages, and also in the ratio of 26 to 34, being that in which the consumption of a passenger-engine, as determined by careful observation, stands to the consumption of a freight-engine. This, in the first instance, gives the total amount of coal consumed in each service, including switching or shunting. In order to arrive at the amount consumed in moving freight-trains on the line, the total amount of engine-mileage made in switching or shunting is noted, and this is divided into two portions, in the proportion in which the passenger-train mileage stands to the freight-train mileage, and the switching is thus allotted to the respective services. The coal consumed in the service is then calculated by allowing 70 miles per ton, and the quantity thus obtained is deducted from the total quantity apportioned to the freight-service. This estimate of 70 miles switching per ton of coal consumed is taken from the observations of the Lake Shore and Michigan Southern Railway, extending over a number of years. It will be seen, therefore, that the results obtained are only close approximations to the absolutely true figures of this subject; but still they are sufficiently close to be valuable as comparisons.

The Table shows that the coal consumed in passenger traffic is less on the Lake Shore line than on any of the others, being 12·8 lbs. per passenger-carriage mile. Taking the average weight of the cars composing the passenger train at 16 tons, this would give a consumption of 0·8 lb. per ton hauled 1 mile; at the same time it is interesting to note that there is a consumption of 1·16 lb. per passenger moved 1 mile. The very large consumption of fuel per ton moved 1 mile in the passenger-service, as compared with the freight-service, is undoubtedly due to the much higher rate of speed of the former, as compared with the latter. Confirmation of this is found on considering the figures applicable to the Hannibal and St. Joseph line. There the consumption per ton-mile in the passenger-service is less than that of either the Canada Southern or Michigan Central, and only very little greater than that of the Lake Shore line; whereas the consumption per ton-mile in the freight-service of the Hannibal and St. Joseph line is very much greater than any of the others, being more than double that of both the Canada Southern or Lake Shore lines. This apparent anomaly is explained by the fact that the speed of the passenger trains on the Hannibal and St. Joseph line is much less than that on any of the others under consideration.

The consumption of fuel in freight-service on the Canada Southern and Lake Shore lines is nearly the same, with a small fraction in favour of the former, while on both lines it is less than on the Michigan Central, or Hannibal and St. Joseph. The amount of fuel consumed in moving 1 ton gross weight (including the fuel consumed in shunting) is barely 2¾ ozs.—a quantity which is surprisingly small. This is on the two first-mentioned lines; while on the Michigan Central and Hannibal and St. Joseph lines it amounts to 4 ozs. and 6·4 ozs. respectively.

In the latter part of the Table the amount of coal consumed in the switching or shunting work of the freight service has been deducted, and that consumed in the work of moving freight-trains on the line of railway only dealt with, with a view of arriving at the quantity consumed in moving 1 ton weight 1 mile. The result arrived at is as follows:—Canada Southern, 2·30 ozs.; Lake Shore, 2·38 ozs.; Michigan Central, 3·52 ozs.; and Hannibal and St. Joseph, 5·76 ozs.

Though it will surprise most people who have not paid particular attention to these questions, to learn that there is sufficient energy in a piece of coal weighing only 2·3 ozs. to move 1 ton weight 1 mile; yet the investigations would not be complete if it were not ascertained what is the total energy of the fuel; what portion of it is used, and what lost.

The units of heat (Fahrenheit) developed in the combustion of 1 lb. of coal are 14133,[1] and as the mechanical equivalent is 772 foot-pounds per unit, the combustion of 1 lb. of coal is equal to 10,910,676 foot-pounds, or 5,455·3 foot-tons (American).

On the Canada Southern Railway, the average of the whole line is equal to a gradient of 5 feet to the mile; this will make the resistance to haulage equal to 11 lbs. per ton, taking the resistance on the level at 9 lbs. per ton; therefore as much energy will be expended in hauling 1 ton 1 mile, as in lifting 11 lbs. 1 mile vertically. In other words, hauling 1 ton 1 mile requires an expenditure of energy equivalent to 5,280 × 11 = 58,080 foot-pounds, or 29·04 foot-tons.

But on the Canada Southern Railway, 1 ton is hauled 1 mile by the combustion of 0·15 lb. of coal, which quantity of coal therefore does work equivalent to raising 29·04 tons 1 foot. At the same rate 1 lb. of coal would raise 193·6 tons 1 foot vertically. But as shown above, the full energy of 1 lb. of coal is 5,455·3 foot-tons; therefore the full energy is to the work effected on the Canada Southern Railway as 100 is to 3·5, and consequently there is a loss of 96·5 per cent. of the energy of the fuel. Though the quantity, 2·3 ozs. of coal, seems extremely small to do the work of hauling 1 ton 1 mile, yet, if all the energy contained in the coal could be utilized and applied to doing work, it would haul 1 ton 28½ miles; while the quantity, 1·86 lb., consumed in moving a passenger 1 mile would, if fully utilized and applied to the transportation of freight, convey 1 ton 353 miles. Few passengers are aware of how much energy is required to make "fast time."

The speed of passenger trains on the Canada Southern Railway was from 35 to 40 miles per hour; on the Michigan Central and

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[1] "A Manual of Rules, Tables, and Data, &c.," by D. K. Clark, M. Inst. C.E., p. 405.

APPENDIX.—TABLE SHOWING CONSUMPTION OF FUEL ON VARIOUS RAILWAYS.

—	Canada Southern.	Michigan Central.		Hannibal and St. Joseph.		Lake Shore and Michigan Southern.
	1881.	1879.	1880.	1879.	1880.	1880.
Total engine mileage (including shunting) .	3,749,701	7,697,051	7,690,051	..	1,995,739	13,586,207
Passenger train-mileage	987,237	1,693,098	1,865,258	414,118	410,368	2,549,081
,, car ,, ...	4,196,466	8,499,352	10,333,529	2,190,243	2,239,970	16,060,832
Average number of cars per passenger train .	4·25	5·02	5·54	5·28	5·45	6·30
Number of passengers moved 1 mile . . .	40,917,987	93,232,430	115,523,789	21,545,368	19,925,041	176,148,767
Average ,, per car	9·75	10·96	11·18	9·83	8·89	10·96
Freight train-mileage	1,775,237	3,687,305	3,658,605	938,095	975,603	7,481,489
,, car- ,, ...	56,915,859	88,384,701	88,491,897	15,715,882	16,864,202	283,588,545
Average number of cars per freight train .	32·06	23·97	24·16	16·75	17·28	37·90
Number of tons of freight moved 1 mile . .	487,965,507	721,019,413	735,611,995	111,987,174	120,665,740	1,851,166,018
Average load per freight car . . . tons	8·57	8·166	8·31	7·13	7·15	6·52
Total amount of coal consumed . . tons	127,270·5	280,160	303,971	69,990	76,898	502,320
Tons of coal apportioned to passenger service	38,072	75,250	84,078	15,190[1]	17,491[1]	102,837
Pounds of coal consumed per passenger train-mile	77·12	88·88	90·15	73·11	85·24	80·68
Pounds of coal consumed per passenger car-mile	18·14	17·70	16·27	13·84	15·63	12·80
Pounds of coal consumed per ton moved 1 mile	1·13	1·10	1·01	0·86	0·98	0·80
,, ,, ,, per passenger moved 1 mile	1·86	1·61	1·45	1·31	1·75	1·16
Tons of coal apportioned to freight service .	89,198	204,910	219,893	54,800[1]	59,407[1]	399,483

Pounds of coal consumed per freight train-mile	100·49	111·17	120·20	116·83	121·77	106·78
Pounds of coal consumed per freight car-mile	3·13	4·64	4·97	6·97	7·04	2·81
Pounds of coal consumed per ton of freight moved 1 mile	0·367	0·57	0·60	0·98	0·99	0·43
Pounds of coal consumed per ton of gross weight[2]	0·168	0·25	0·27	0·40	0·41	0·17
Average miles run by engines per ton of coal	29·46	27·47	25·30	..	25·95	27·04
Total expenses (exclude renewals and taxes) $	2,550,223·11	..	..	1,223,421·97	1,304,590·08	..
Passenger ,, (exclusive ,, ,,) $	584,148·13	..	..	414,150·20	439,295·75	..
,, ,, per train-mile $	0·59·17	..	..	1·01	1·07	0·92·29
,, ,, per car-mile cents	13·92	..	..	18·9	19·6	14·65
Freight ,, $	1,966,074·98	..	..	809,271·77	865,294·33	..
,, ,, per train-mile $	1·1075	..	..	0·863	0·887	1·0767
,, ,, per car-mile cents	3·45	..	..	5·15	5·13	2·84
Average ,, of tons of freight moved 1 mile cents	0·40	..	..	0·72	0·72	0·43
Coal consumed in freight shunting alone, tons	9,025	22,698	20,915	..	6,122	35,282
,, ,, by freight-trains alone tons	80,173	182,212	198,948	..	53,285	364,201
,, ,, per freight train-mile lbs.	90·32	98·8	108·7	..	109·23	97·36
,, ,, per freight car-mile lbs.	2·81	4·12	4·5	..	6·32	2·57
,, ,, per ton of freight moved 1 mile lbs.	0·32	0·50	0·54	..	0·88	0·39
Coal consumed per gross ton moved 1 mile lbs.	0·15	0·23	0·24	..	0·37	0·155

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[1] These quantities are found from the value of fuel apportioned to the passenger and freight traffic in the Annual Report.

[2] The weight of the cars is assumed at 10 tons each.

Lake Shore lines from 33 to 36 miles per hour; and on the Hannibal and St. Joseph line about 25 miles per hour. The speed of freight trains on all the lines was between 15 and 20 miles per hour.

The position of acting Chief Engineer, which the Author until recently occupied on the Canada Southern Railway, enabled him to obtain the information in regard to gradients required to make the foregoing investigations; but the like information has not been obtainable for the other railways under consideration, and therefore it is not possible to say whether they waste more or less of the energy of the fuel consumed. A comparison on a similar basis with English railways would be interesting and valuable, but the necessary data do not seem to be available. These figures clearly indicate how much yet remains to be done in economizing the energy developed in the combustion of coal. An engine which wastes 96½ per cent. of the energy with which it is supplied cannot be called perfect.

The Table also shows the cost of the service performed, worked out in a similar manner as the consumption of fuel.

(*Paper No. 2103.*)

"Electric-Lighting for Railway-Trains."

By William Stroudley, M. Inst. C.E.

A Pullman train has been running for some years between the Victoria and Brighton stations of the London, Brighton, and South Coast Railway, lighted by glow-lamps supplied by accumulators, the accumulators being charged, whilst the train is at rest, from a dynamo-machine driven by a gas-engine. Although this system answers very well by a particular train, it is not one that could be generally adopted, as the difficulty of arranging the trains in certain positions and for a given length of time, to re-charge the accumulators, would be too great. The Author, therefore, in conjunction with Mr. Houghton, the Electrical Engineer to the Brighton Railway Company, devised a system by which the light is generated by the motion of the train; the dynamo being placed in the brake-van with a number of accumulators to store the necessary amount of electricity, so that the lamps shall continue to give light when the train is at rest. The dynamo-machine is driven from a counter-shaft under the van, and connected by two leathern chain-belts with one of the axles of the van. Two belts have been adopted to reduce the risk of failure; and a dynamo-machine is also being made to be driven by two separate belts, one at each end of the spindle. This diminishes the risk of failure of the belts to a minimum.

A simple apparatus, similar to that provided for moving the cross-slide of a planing-machine, is also provided for tightening the driving-straps, by moving the counter-shaft in a diagonal direction so as to brace up all the straps at one time. Two feeding-screws, geared together by worm-wheels, preserve the parallelism of the counter-shaft.

The cables are passed by preference along the top of the carriages, in a piece of timber having two grooves, one groove for each cable. All the compartments of a carriage are provided with two lamps, each lamp having separate connections, so that the breakage of one lamp does not interfere with the work of the other.

To enable the carriages to be readily separated and attached, the wires are brought down to a suitable height, about 1 foot on either side of the centre of the ends of the carriage; and by a simple coupling, one part consisting of a conical barrel, and the other of a plug similar to an ordinary stop-cock, the barrel being attached to one wire, and the plug to the other, the carriages can be coupled no matter which ends come together. These couplings are held together by four clips or springs which, after the plug is pushed into position, retain it there; but they will yield and allow it to escape should the porters forget to uncouple them when shunting or moving the carriages. Thus the arrangement does not introduce any difficulty or parts that the ordinary porters cannot understand. As the brake-vans are uniform, any one part will fit the other, and any van or coach can be joined to another in the same way as with the ordinary screw-couplings and buffers. This system offers many advantages as compared with the stationary arrangement. The light is turned on and off by the guard.

The dynamos at present in use are of the old Brush type, having an electromotive force of 45 volts, and furnishing a maximum current of 56 amperes.

The two pairs of collecting-brushes are mounted on a rocking-frame, and in their normal position stand free from the commutator. They are so arranged that one pair shall collect the electricity when the train is travelling in one direction, and the other pair when it is going the opposite way. Parallel with, and beneath the axle of, the dynamo is a lever, having at one end a soft-iron armature immediately under the pole-pieces of the field-magnets of the dynamo, and connected at the other end to the brush-frame. This lever carries a small spindle parallel with it, one end having a cross-bar round which passes a strap that in turn passes round the dynamo-axle. The other end of this small spindle is joined to an upright rod, having at its upper end a loop which embraces a curved bar on the brush-frame. Thus when the dynamo-axle begins to rotate it moves the strap and cants the cross-bar, partially rotating the small spindle, and bringing the loop of the upright rod to the one or the other extreme position on the curved bar of the brush-frame. Consequently, when the lever moves on its fulcrum, it causes either one or the other pair of brushes to come into operation, according to the position of the upright rod.

The automatic contact-maker consists of a vessel containing mercury, with a vertical tube of vegetable fibre at each end, having a coarse-threaded screw, the axis of which is driven by a belt from the revolving dynamo-axle. According as the screw-blade is driven in one direction or the other, the mercury is caused to rise in the one or the other tube; and when the dynamo-axle attains sufficient speed to put the mercury in electrical contact with an adjustable screw at the upper end of the tube, the shunt-circuit of the dynamo is joined up, the field-magnets are charged, and they attract the armature on one end of the before-mentioned lever, depressing the other end, and thereby putting one pair of brushes on to the commutator. The putting on of the brushes joins up the main-circuit in the ordinary manner, and causes the accumulators to be charged. As the speed of the train slackens, and the dynamo-axle does not revolve so fast, the mercury in the tube gradually descends, and when it falls sufficiently to break contact with the adjustable-screw, the shunt-circuit is broken, and the armature on the lever falls from the poles of the field-magnets, thus removing the brushes from the commutator and breaking the charging-circuit.

The lamp-circuit and the accumulator-circuit are joined to the dynamo in such a manner that when the lamps are alight and the train is in motion, the current from the dynamo feeds them, the accumulators regulating the light. When the dynamo is out of circuit, the lamps are supplied direct from the accumulators. If the dynamo does not supply sufficient current for the lamps, the balance is given out by the accumulators. If, however, the dynamo furnishes an excess of current, the surplus is stored in the accumulators.

In a suburban train of eleven vehicles on the London, Brighton and South Coast Railway, there are thirty-two incandescent-lamps of 16-candle power each. These absorb 40 amperes of current.

The accumulators are stated by the manufacturers (the Electrical Power Storage Company), to hold 500 amperes, and when fully charged there is a reserve of electricity sufficient for eight hours. This particular train has been running since the 19th of December, 1883. It was computed that during the first eleven months of the time, the train made 2,352 trips, and ran 27,322 miles. A train fitted up according to this system, on the South-Eastern Railway, has travelled a longer distance.

The various installations of this system have, up to the present time, given every satisfaction. The apparatus can be used for several weeks with no attention beyond lubrication. At the end of that period, the commutator-brushes require adjusting, which can be done in five minutes, and then the apparatus will be ready for use again for a similar period without needing attention beyond that already mentioned. The weight of the apparatus is about 2 tons.

Three trains have been fitted with this system on the Brighton Railway, and one train upon the South-Eastern Railway.

The Paper is accompanied by a tracing from which Plate 4 has been engraved.

(*Paper No. 2050.*)

"The Karachi Waterworks."

By James Strachan, M. Inst. C.E.

The province of Sind, of which Karachi is the chief town, has an area of some 57,000 square miles, of which about one-half is either a desert or uncultivated land. It lies between north latitudes 23° and 28°, and east longitudes 66° and 71°. It is bounded on the north by Beloochistan, the territory of the Khan of Khelat, and by portions of the Punjab and of the Bhawulpore State; on the south and south-west by the Runn of Kach and the Indian Ocean; on the east by the Rajput States of Jaisalmere and Marwar; and on the west by Beloochistan. Sind is a flat country, the only mountains being the Khirthar range on its western boundary, and the tract of hills known as Kohistan in the Karachi collectorate. The Pubb hills, near Karachi, are the continuation of the Khirthar chain, which extends in a southerly direction for 90 miles till it meets the sea at Cape Monze. It is amongst the ravines of this range of hills that the Hubb, the only permanent river in Sind except the Indus, takes its rise. The other rivers or mountain-torrents of importance, which are not tributaries of the Indus, are the Lyari and Malir. The former rises among the hills of Kohistan, a few miles north of Karachi, and falls into the harbour close to the town. During the greater part of the year the bed of this river is dry, but after a heavy rainfall a considerable body of water flows in the channel for a day or two. The Malir River is, like the Lyari, a torrential stream, dry for the greater part of the year. It also has its rise in the Kohistan district, or mountainous tract between Karachi and Sehwan, and after a south-westerly course of 60 miles falls into Karachi Bay by a creek, a few miles east of the town.

The town of Karachi lies about 12 miles north-west of the most westerly mouth of the Indus, in north latitude 24° 51′ and east longitude 67° 2′. When taken by the British in 1839 it was but little more than a fishing village, and in 1842, when finally ceded to this country, the inhabitants resided in what is now known as the old town quarter. It was miserable and dirty in the extreme, with few roads and no means of locomotion. Passengers, on arriving from seawards, made the best of their way in small boats up the harbour, which, near the town, was little better than liquid mud. When the boats could proceed no further the passengers were carried on men's shoulders till dry land was reached. Since then, however, the city has advanced with rapid strides. The harbour,[1] which has been enlarged and deepened, is now one of the best in India, and is connected with the town by an excellent road 2 miles long, called the Napier Mole, while other good metalled roads lead to every part of the town and camp. Conveyances are easily obtainable, and handsome buildings are springing up on every side. The population, which was 14,000 in 1842, and 56,000 in 1872, had risen to 74,000 souls in 1881, and at the present time is estimated to be close upon 100,000. The foreign trade of the place has increased in a still more striking manner. Five years ago the annual value of the imports was about £530,000, now it is £1,220,000. Then, the value of the exports amounted to £1,000,000, now it is £2,660,000. The coasting trade has also vastly increased of late years.

Karachi is divided into two portions, namely, the native quarter, which is only a few feet above high-water mark, and the European quarter, including the Cantonments, which lies more inland, and upon much more elevated ground. It is surrounded on the south and west by extensive mud-flats raised to the level of 1 foot to 3 feet below high-water mark by a deposit of salt mud, on which mangroves and soda-plants are the only vegetation; and on the south by sandy dunes on the heights of which the goat's foot creeper grows, and gives stability to the shifting sands.

The south-west wind prevails during the hottest months of the year, and this, added to the low situation of the town, and the proximity of the marsh-lands, makes the climate of Karachi moist and humid; but taken as a whole it may be considered as the most salubrious in Sind.

The rainfall at Karachi is exceedingly scanty and precarious, being seldom more than 6 inches per annum. In some years a rainfall of over 20 inches has been registered, as in 1851, 1869, and again in 1878, when 26 inches, 28·45 inches, and 25·50 inches were gauged respectively, while at other times a couple of years will pass with almost no rain at all. The south-west monsoon never extends beyond Lakhput bunder on the coast of Kach, while the north-west monsoon does not quite reach Karachi. A Table of the rainfall and temperature is given in Appendix VI.

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[1] Minutes of Proceedings Inst. C.E., vol. xliii. p. 1.

The Malir river is called by different names along its course. At its source it is known as the Vuddia, in the middle as the Goorban, where it is joined by a small torrent called the Kuttagee, and, after receiving the tributary waters of several other streams, it falls into the sea as the Malir. Although this river is dry for the greater part of the year, yet water is readily obtained at any time in the sandy bed by digging down a few feet below the surface, a method which the cultivators on both banks have adopted. By this means, and by shallow channels artificially excavated, water is run off for irrigation purposes for miles along the serpentine course of its bed. At a depth of from 10 to 30 feet below the bed water is plentiful. The Malir river when flooded, as it is by periodical heavy rains, discharges a large body of water with such velocity that all obstacles are swept before it. The country drained by this river is about 600 square miles in area, and is very sparsely populated. The surface between Malir and Karachi is generally even. The soil is composed of alternations of gravel and sand, and a pale alluvium loam and gravel. In the vicinity of the river it is a light loam containing a large proportion of micaceous particles. The gravels in many places are consolidated by the admixture of carbonate of lime from the limestone pebbles. The geological formation of this tract differs in no material respect from that generally prevailing in Lower Sind, and is identical with what has been termed the Gaj, or Miocene group, intermingled with the Munchars or Pliocene. The general section in descending order is: 1, conglomerate; 2, clay and sandstone; 3, upper bone-bed; 4, sandstones; 5, a pale yellow arenaceous limestone more or less pure, followed in some places by a thin stratum of nummulitic limestone. The sandstones and limestones, found at intervals in the trenches between the distributing reservoir at Karachi and the wells at the Malir river, form beds nowhere exceeding a thickness of 15 feet. The former are either gray, brown, or slatey, more or less compact, and generally having a tendency to cleave into loose blocks, or when not sufficiently compact, friable or decomposing on the surface. The latter are equally variable in character, being in some places, when not arenaceous and compact, of a fine texture, homogeneous and often crystalline, abounding with a minute spheroidal foraminifer known as "alveolo." In one place along the line of works, 8 miles from Karachi, variegated sandstones and marl occur, below which is a bed of dark and plastic clay mixed with siliceous particles. Shelly limestones are met with about 6 miles from Karachi, interstratified with sandstone and calcareous grit, superimposed on which are thick beds of gray, rather sandy indurated clay, somewhat calcareous. The conglomerates extend to about 4 miles from Karachi, where an elevated ridge crops out and divides the basin of the Malir from that of the Lyari.

The rainfall being so scanty the supply of drinking-water is deficient in quantity, and likewise defective in quality.

The sources of water-supply from the period of the occupation of the British, up to the time when the works referred to in this Paper were completed, were, with one or two exceptions, a few wells in gardens, and shallow wells sunk in the bed of the Lyari river several hundred yards higher up the stream than high-water mark. These latter wells, which were only shallow holes in the river-bed, constituted, in the opinion of the majority of the natives, the only method of obtaining sweet water in the place. When the hole had been opened for about twenty days or so, the water in it became so brackish that it was undrinkable, and as soon as this happened another hole was scraped out, which for a few days more afforded sweet water, and so on. In this manner the inhabitants of Karachi obtained their supply, and it satisfied them. The late Sir Bartle Frere, when Commissioner in Sind, said of the water obtained from the best of the wells in the place, that "though the permanent residents get used to it, and it does not disagree with them, it has on all new comers the effect of a weak solution of Epsom or Cheltenham salts. Hardly any new arrival escapes inconvenience for the first few weeks of his stay, and to some persons of delicate organization, and on invalids, the injury is considerable and permanent."

The quality of the Karachi water attracted the attention of the authorities soon after the conquest, and Sir Charles Napier, being of opinion that the want of good drinking-water in Karachi was the one great drawback to the place as a large military station, proceeded to take steps to supply the want. From that time to the date on which the works referred to in this Paper were commenced, many schemes were proposed and considered, notably those of Captain Baker, R.E., in 1845, Captain (now General) De Lisle, R.E., in 1854, Mr. John Brunton, M. Inst. C.E., in 1860, Mr. Newnham, Chief Resident Engineer of the Sind, Punjab and Delhi Railway, in 1864, and Major-General Merriman, R.E., in 1868. These schemes were all practicable, and with the exception of that of Mr. Newnham, which comprised a boat-canal from the Indus, suitable for a municipality to undertake. Notwithstanding the importance, both to the Government and to the public, of an improved water-supply, difficulties were continually arising between the Government and the Municipality regarding the share of the cost which each should bear, and what assistance the Municipality should receive from Government in the execution of the works. Eventually it was intimated to the Municipality that the work was one with which the Government could not be concerned, further than to grant permission to the Municipality to raise a loan for the purpose.

In 1873 the Author, on being appointed Engineer to the Municipality, proceeded to bring the subject of water-supply prominently before the Municipal Commissioners; and, after having carefully considered the various proposals which had been made from time to time, he submitted the scheme which has been carried out. The majority of those persons who had in former times proposed schemes for supplying Karachi with water, had adopted the Malir river as the source of supply; but not until 1868, when Colonel Merriman carried out an important and conclusive test, was the public mind satisfied as to the underground capabilities of that river.

The test consisted in ascertaining, by means of pumping, at what level below the dry bed of the river 1,200 gallons of water per minute could be obtained, this being the quantity necessary to supply Karachi with about 22 gallons daily per head of population reckoned at about 80,000. The excavation for the trial well was carried down to the depth of 10 feet below the water-level, or 24 feet below the surface-bed of the river, and a trench 180 feet long by 9 feet wide at the bottom was excavated to a depth of 20 feet below the surface-bed. The pumps employed were two of Gwynne's B B centrifugal pumps, and it was found that these, when throwing over 1,200 gallons per minute, failed to reduce the water in the well below 6 feet under the water-level. As the year 1868 was not in all respects as favourable a year as could have been desired for an experiment of this nature, inasmuch as the average annual rainfall in Karachi for the three years preceding was 7·73 inches, while the average annual fall from 1856 to 1866 was 6·81 inches, it was manifest that an ample margin should be provided for drier seasons than that of 1868 in fixing the level of the main pipe. When, therefore, on the engines being stopped it was found that the level of the water by the gauges in the well and trench rose from 5 feet in depth to 4 feet in nine and a half minutes, equivalent to a supply of 1,272 gallons per minute, or rather more than what was wanted, it was recommended that the top of the main delivery-pipe should be laid at the level of 6 feet below the water-line above referred to.

In all previous schemes it had been proposed to connect the wells with the distributing reservoir by pipes; but owing to the high price of iron at the time, the Author resolved to take advantage of the excellent stone, easily procurable in the neighbourhood, to construct a masonry conduit, instead of using cast-iron pipes. To accomplish this it was necessary to choose another site for the wells, 2 miles higher up the river than the one at which Colonel Merriman's experiment had been carried out. The Municipality having approved of this scheme, the Author was instructed to prepare detailed plans and estimates for submission to Government. These were in due time approved, the total estimate of the work amounting to 1,200,000 rupees. The daily quantity of water which it was estimated could be supplied to the inhabitants of Karachi was about 25 gallons per head of the population estimated at 80,000.

Difficulties, chiefly of a financial character, having delayed the commencement of the work till the year 1879, the Author recommended that in order to bring the estimates within the means of the Municipality, the large 24-inch main, and several sub-mains, which formed part of the original scheme, should not be laid at first. By thus reducing the capacity of the main to 8 gallons per diem per head of population, and shortening the length of distributing-pipes, the cost of the works was brought down to within the means which the Municipality had in hand, namely, 850,000 rupees. These suggestions were adopted, and on the 21st of February, 1880, the foundation stone of the distributing reservoir was laid by Sir Richard Temple, then Governor of Bombay, who had taken great interest in the proposed work, and who, by his personal influence with the Municipal Commissioners, had done much to bring about its commencement.

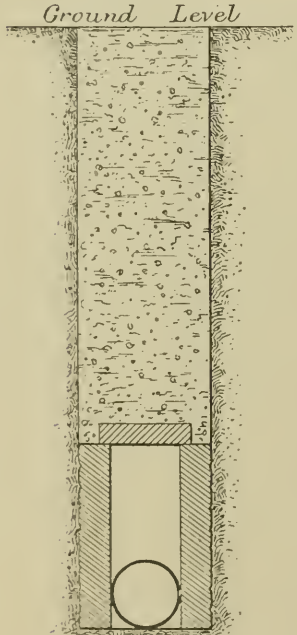
The supply of water is obtained from two wells situated on the right bank of the Malir, about 7 miles from the Landi station of the Sind, Punjab, and Delhi Railway, and about 16½ miles in a bee-line from Karachi (Plate 5, Figs. 1 and 2). As the river in times of flood suffers much from the erosion of its banks, it was deemed desirable that the wells should be so situated as to be sufficiently removed from any such action. They were accordingly sunk 400 feet apart, and at a distance of 1,000 feet from the river bank. They are each 40 feet in diameter inside, and were excavated to their full depth of 36 feet without the aid of pumps of any kind, on account of the favourable position of the Thudda river, which is a tributary of the Malir. This river crosses the line of conduit about 3 miles from the wells, and at such a level as enabled the whole of the water from the excavation of the wells to be discharged into it. The wells are steined 3 feet thick with rough ashlar blocks, 3 feet by 1 foot 6 inches by 1 foot, laid header and stretcher alternately, set dry with beds and joints roughly dressed to facilitate the inflow of the water. The walls are carried 23 feet above ground, and are finished with an ashlar cornice.

Between the wells and the Thudda river, (Plate 5, Figs. 3, 4, 5, 6 and 7) the water is conveyed partly in pipes and partly in a masonry conduit. The latter connects the junction-tank with the Thudda, a distance of 2,380 feet, and was built before the pipes, which extend from the wells to the junction-tank, were laid. The excavation for this latter length was next proceeded with, and 24-inch cast-iron pipes were laid in the bottom as the excavation advanced. These pipes had all their joints turned and bored to facilitate their being laid under water, which, as the work advanced towards the wells, rapidly increased in quantity, and was as quickly discharged by the trench, which was 4 feet wide as far as the junction-tank. From this point to the Thudda the water was carried off by the conduit. The total quantity of water discharged daily into the Thudda river, on the completion of the excavation of the wells, was 2,500,000 gallons. The stratum through which the wells were sunk was coarse, compact, river gravel, which neither here nor in the excavation for the pipe-trench required shoring of any kind, but stood with perpendicular sides for many months, including one monsoon. In the centre of each well a cast-iron valve-shaft (Plate 5, Fig. 3) 4 feet in diameter is erected in seven lengths, bolted together by 1½-inch bolts. The 24-inch cast-iron pipe forming the continuation of the conduit is connected with the valve-shaft at the bottom length, where a 24-inch sluice-valve is fitted for regulating the flow of water through the pipe and in the conduit. The valve-shaft has three inlets at intervals of 6 feet, through which the water is drawn from the well. These inlets are each 24 inches in diameter, fitted with sluices faced with gun-metal, and are worked by means of wrought-iron rods, supported by cast-iron pillars fastened to the gangway on the top of the valve-shaft (Plate 5, Fig. 4). The pillars of the sluice on the discharge pipe are each provided with a brass index-plate and index, registering the number of inches the valves are open. The junction tank, into which the pipe from the wells discharges, is at a distance of 6,880 feet from the latter, and is a small circular well of rubble masonry, 10 feet in diameter, with the sides rendered in cement ½-inch thick.

For a distance of 5,000 feet from the wells, the pipes are enclosed in a masonry culvert 2 feet 6 inches wide by 6 feet high built dry. The pipe is by this means protected from the great weight of the superincumbent mass of earth, and at the same time the water in the trench finds an easy means of access to the wells (Fig. 1).

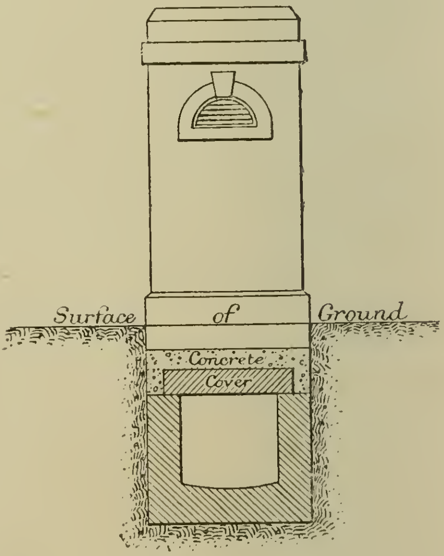
The masonry conduit commences at the junction-tank, and has, for nearly 9½ miles, a sectional area of 7·31 square feet with a fall of 2 feet to the mile. The remaining portion of the conduit has

FIG. 1.



Scale ⅛ inch = 1 foot.

FIG. 2.



Scale ⅛ inch = 1 foot.

a sectional area of 5·62 square feet with a fall (excepting two short lengths where it is greater) of 3·91 feet per mile. The total length of the conduit is 16 miles 20 chains, and it ends at the inlet well near the distributing reservoir at Karachi. The conduit is covered in for the whole distance, and is provided with ventilating shafts and manholes, some placed 1 mile apart where the excavation is shallow, and others ½ mile apart where the depth of the conduit from the surface of the ground is above 15 feet. These ventilating shafts have Moulmein teak doors giving access to a wrought-iron ladder for the convenience of inspection. The cross-section of the conduit and the elevation of the ventilating shaft are shown by Fig. 2. The side walls and invert are built of rubble masonry, no stone being of less breadth of bed than 12 inches. The whole of the inside of the conduit up to the underside of the covers is rendered in cement ½-inch thick, of the proportion of 1 part of Portland cement to 3 parts of good clean river sand. The covers are 8 inches in thickness with 6 inches of concrete above.

Where the line of conduit is crossed by water-courses at about the same level, the conduit is carried underneath by siphons. The siphon under the Thudda river is formed of a double row of 24-inch cast-iron pipes 500 feet long, laid about 8 feet under the bed of the river, with wells 10 feet in diameter at each end which act as ventilating shafts. Branches 12 inches in diameter, closed with blank caps, are cast on the centre length of each row of pipes, to afford an easy means of removing any deposit which may accumulate. The conduit is carried in several instances under nullahs by siphons built entirely of masonry and concrete (Plate 5, Fig. 8). The concrete was composed of 5 parts of broken stone, 2 parts of river sand, and 1 part of lime. The masonry of the side-walls was common rubble, and that of the arch block-in-course. The whole of the internal walls including the soffit are rendered in cement in a similar manner to the walls of the conduit. Plate 5, Fig. 9, shows another siphon where 24-inch cast-iron pipes were used. At each end of the pipes in the wells, cast-iron creeping flanges have been fixed in the stonework with cement-plaster of the same description as that used for the wells.

As the supply of water was known to be much in excess of the quantity which could be used in the town, for at least some years to come, the Author suggested that the surplus should be utilized for irrigation purposes. Accordingly at about 2 miles from Karachi, where arable land was available, and where the level of the ground was suitable, an irrigation well was built (Plate 6, Fig. 10). In this well two 24-inch valves are placed at right angles to each other; one valve on the direct line of conduit, and the other on the pipe leading to the irrigation channel. They are worked by bevelled pinions on the same spindle, and so arranged that when one valve is opened the other is shut. In this manner any chance of damage to the conduit from the backing up of water is avoided. The average daily supply of water from the wells is 2,000,000 gallons, and as the average daily consumption does not at present exceed 1,500,000 gallons, 500,000 gallons remain to be disposed of on the land. This quantity is equal to

1,666,666 cubic yards per annum, and allowing 8,000 cubic yards to be required per acre for two crops per annum, the available quantity will be sufficient to irrigate about 146 acres. At present only 30 acres have been placed under irrigation, but it is proposed to increase this area from time to time.

The inlet-well, into which the water carried by the conduit is discharged, is built adjoining the distributing reservoir, and acts as a second chamber to it. It is 20 feet in diameter, and is lined with ashlar 2 feet in thickness set in Portland cement mortar up to ground-level; above that the walls are 18 inches in thickness of block-in-course masonry carried to a height of 24 feet, and finished with a terraced roof.

The distributing reservoir (Plate 6, Figs. 11 and 12), built on the side of a hill about 1 mile to the east of the Cantonments, is 200 feet long by 150 feet in width; the water-surface is 10 feet 9 inches above the floor. The foundation, which is on clay shale, consists of a layer of concrete 12 inches in thickness over the whole area of the excavation, with a course of large flat-bedded rubble 8 inches thick laid above, and on this the walls and piers are built. The external walls and piers are of coursed rubble 18 inches in thickness. The whole area is covered by fifteen segmental arches of block-in-course masonry 15 inches thick, each having a span of 12 feet $\frac{3}{4}$ inch. The piers are also arched. Two cast-iron ventilators 9 inches in diameter are fitted into each compartment. The spandrels of the arches on the top are filled in with rubble masonry. The whole is covered with concrete to the height of 1 foot above the upper side of the key-stones, and a layer of 2 feet of earth has been spread on the top of the concrete. The whole of the inside of the reservoir, including all arches and piers to a level of 3 inches above the water line, is rendered in cement 1 inch thick, composed of 3 parts of sand to 1 part of Portland cement.

The levels of the delivery-pipes in the reservoir and inlet-well correspond, and are 10 feet 3 inches below the top-water level of the reservoir. This is 62·35 feet above mean sea-level, and 55 feet above the average level of the ground floors of the houses in the town; there is therefore sufficient head to supply water to r floors of all the houses.

in delivery-pipe is of cast-iron, 24 inches in diameter, o branches, one branch from the inlet-well, and the he reservoir. These pipes are fitted at the ends in reservoir respectively with 24-inch sluice-valves, so be supplied to the town direct from either. A cast-iron pipe, 24 inches in diameter, connects the inlet-well and the reservoir, and is controlled by a sluice-valve on the end in the well. The extremity of the pipe in the reservoir is bell-mouthed.

The arrangements for cleansing both the inlet-well and the reservoir are similar. The cleansing-pipe, which communicates at some distance from the reservoir with a drain, is laid 2 feet below the level of the main delivery-pipe, and like it has one branch from the well and one from the reservoir, ending in each case with a sluice-valve of the same description as those on the main delivery-pipes.

Immediately behind the valves on the cleansing-pipes, in both the inlet-well and the reservoir, flanges have been cast forming seats for the overflow-pipes, which are also 24 inches in diameter; these are bolted to the flanges below, and are bell-mouthed above. By this means the overflow-water falls into the same drain as the water from the cleansing-pipe. The pipes communicating with the inlet-well and reservoir have creeping flanges, which are let into the masonry flush with the face of the wall. The whole of the connections have proved satisfactory and perfectly watertight. All the sluice-valves, both in the inlet-well and the reservoir, are faced with gun-metal, and are adjusted from the valve-house by wheels fitted on the end of wrought-iron rods, working in pillars firmly fixed to the floor. These pillars are provided with brass indices showing the number of inches each valve is open.

As the connecting-pipe, between the well and the reservoir, and the end of the main delivery-pipe are so close together, it was deemed expedient to build a partition-wall between them, dividing the reservoir for two-thirds of its length into two compartments, in order to ensure a constant circulation of the water.

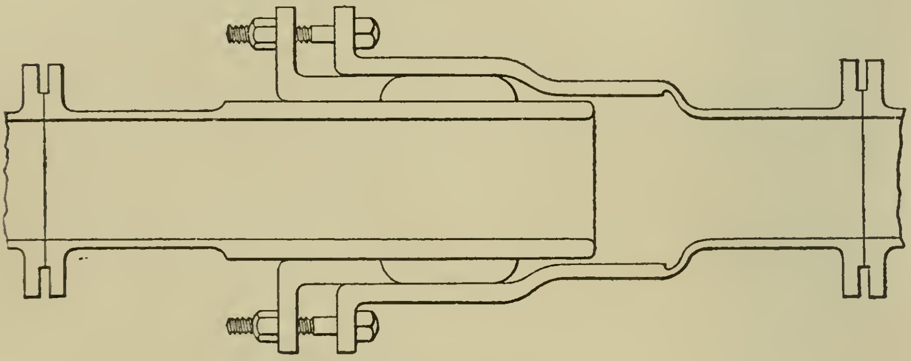
The main delivery-pipe, after leaving the inlet-well and reservoir, is continued of a diameter of 24 inches for a distance of 150 feet, when it bifurcates, one branch, 12 inches in diameter, being taken through the Cantonments and civil lines, past the railway quarter and old town, to the shipping at Keamari; and the other, 21 inches in diameter, by a straight route through the Sadar bazaar to the native town. This latter main is the one the laying of which was deferred at the commencement of the works for financial reasons. No sooner, however, had the partial supply of 8 gallons daily per head been introduced, and the people had tasted the benefits of wholesome water, than a general clamour arose for the extension of the scheme to its original limits. Accordingly, on the 11th of June, 1883, the Municipality sanctioned the raising of a loan of 289,000 rupees to complete the scheme. The additional works consist mainly in supplying and laying pipes, and in the erection of services at various places in the town.

The distributing-pipes are laid in the usual manner along the principal streets. Of the pipes used, 80 per cent. have turned and bored joints, and 20 per cent. have lead joints; a detailed statement of the cost of laying and jointing the various sizes of pipes is given in Appendix I.

Hydrants of the ordinary pattern are placed at varying distances, in some streets 200 feet apart, to facilitate road watering, and in others 400 feet apart. The total number of hydrants is six hundred and thirty-five.

The supply is in great measure a public one, as up to the present time only about four hundred houses have been connected by service-pipes with the main. Public street-services have been erected at convenient spots throughout the entire camp and town of Karachi, whence water can be obtained at any time.

Fig. 3.



Scale 1 inch = 1 foot.

The public hand-services (Plate 6, Figs. 13 and 14) are twenty-eight in number, and consist of fountains furnished with $\frac{3}{4}$-inch brass thumb-push cocks. As water is almost universally carried from the wells in large earthern pots or gurrahs, which are very brittle, the platform beneath each cock is lined with wood, hollowed to fit the gurrah, which rests on it while it is being filled.

For those who live at a greater distance, and who cannot send a member of the family to draw water as those do who live in the immediate vicinity of the street fountains, the bhistie with his bullock puckhal is required, and for him the bullock service has been provided. This service consists of a horizontal pipe having two branch pipes cast on it, sufficiently far apart to fit the mouths of the bhisties' puckhals, which are two leather bags, one on each side of the bullock. The horizontal pipe is supported on stone pillars, of such a height and distance apart as enables the bullock to stand comfortably underneath it. The bhistie opens the cock by lifting the weighted lever, and while it is thus held open the water flows through the two branches of the pipe, filling equally both puckhals (Plate 6, Fig. 15). The total number of bullock services is forty. Carts requiring water for any purpose can fill from the ordinary street stand-post, of which there are fifty-five.

Special supplies of water have been furnished to the Sind, Punjab and Delhi Railway Company's workshops, the arsenal, the jail, to all the troops in garrison, both European and native, and to the various mills and factories in Karachi. A 6-inch branch has been taken along the Napier mole to Keamari, a distance of 2 miles, for the supply of the shipping in the harbour, the Keamari station of the Sind, Punjab and Delhi Railway, and the hydraulic machinery connected with the Merewether pier. A branch-pipe has also been laid along the Merewether pier, whence steamers lying alongside can fill their boilers at any time. In carrying the pipe along the Napier mole, a wrought-iron screw-pile bridge of thirty spans of 40 feet each had to be crossed. The pipes were suspended below the cross-girders, and to provide for expansion and contraction the joint shown in Fig. 3 was employed.

The stone used on the works was that commonly quarried in the neighbourhood, and is an arenaceous limestone composed as follows:—

Clay	5·60
Free sand	9·30
Oxide of iron	2·00
Carbonate of lime	80·00
Carbonate of magnesia	1·90
Moisture and loss	1·20
Total	100·00

The total cost of the scheme has been 1,150,000 rupees; and as the maximum daily supply of water is 2,500,000 gallons, the cost of 20 gallons per day is 9 rupees. The total cost of the reservoir was 67,000 rupees, or £6,700. It is capable of containing 2,000,000 gallons of water. The cost per 1,000 gallons of water stored is therefore £3 7*s*.

The Municipality has not as yet imposed any general water rate on the inhabitants; those who take service-pipes into their houses pay a monthly fee according to the size of the connection; R. 1 for a ½-inch connection, Rs. 2 8a. for a ¾-inch, and Rs. 5 for a 1-inch connection.

The works were commenced on the 18th of February, 1880, and were formally opened on the 21st of April, 1883. The work connected with the extension of the distributing-pipes was commenced in July, and finished in October 1884.

The contractors were: Messrs. Burn and Co., Calcutta, for the conduit and wells; Messrs. Meherally and Lallu Gugoo of Karachi for the reservoir; the ironwork was supplied by Messrs. R. Laidlaw and Son of Glasgow; and the work of laying the distributing-pipes, and fixing the meters, valves, &c., was entrusted to Messrs. T. Cosser and Co. of Karachi.

The lengths and weights of the different mains are given in Appendix II.

The Paper is accompanied by several small-scale drawings, from which Plates 5 and 6 and the figures in the text have been engraved.

APPENDIXES.

APPENDIX I.

	Per	Rs.	A.	P.
21-inch main.—Supplying at Karachi	ton	74	11	7
,, Landing, carriage, dues, &c. . .	,,	4	2	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	2	8	0
18-inch main.—Supplying at Karachi	ton	74	11	7
,, Landing, carriage, dues, &c. . .	,,	3	10	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	2	0	0
15-inch main.—Supplying at Karachi	ton	74	11	7
,, Landing, carriage, dues, &c. . .	,,	2	10	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	1	10	0
12-inch main.—Supplying at Karachi	ton	74	11	7
,, Landing, carriage, dues, &c. . .	,,	2	10	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	1	4	0
9-inch main.—Supplying at Karachi	ton	75	10	2
,, Landing, carriage, dues, &c. . .	,,	2	8	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	1	0	0
6-inch main.—Supplying at Karachi	ton	78	10	9
,, Landing, carriage, dues, &c. . .	,,	2	8	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	0	14	0
3-inch main.—Supplying at Karachi	ton	81	11	5
,, Landing, carriage, dues, &c. . .	,,	2	4	0
,, Laying and jointing, including excavation and refilling trench	lineal yard	0	10	0

APPENDIX II.

Diameter of Main.	Length.	Weight.	Diameter of Main.	Length.	Weight
Inches.	Lineal yards.	Tons.	Inches.	Lineal yards.	Tons
24 . . .	68	19	7 . . .	1,266	62
21 . . .	2,356	530	6 . . .	9,675	394
18 . . .	2,308	424	5 . . .	10,725	343
15 . . .	528	77	4 . . .	8,838	209
12 . . .	2,646	270	3 . . .	12,375	190
9 . . .	3,336	237	2½ . . .	1,860	24

APPENDIX III.

Cost of Materials and Labour in Karachi.

The following statements of the cost of materials and labour in Karachi may be found useful.

Ashlar per 100 cubic feet, exclusive of European superintendence, plant, &c.:—

	Rs.	A.
Quarrying stone, including all materials and labour, per 100 cubic feet	5	0
Carriage of stone (5 miles lead), ten carts, each cart carrying 10 cubic feet at R.1¼ per cart	12	8
Dressing (one man will dress 4 cubic feet per day at R.1 2a. per day)	28	2
Setting (one man will set 10 cubic feet per day at R.1 2a. per day)	11	4
Nowgunnies, coolies, &c.	6	10
Sand, lime and water	3	8
Total for 100 cubic feet of ashlar	67	0

Or equal to £1 16*s*. 2½*d*. per cubic yard, taking the value of the rupee at par.

Block-in-course masonry per 100 cubic feet, exclusive of European superintendence and plant:—

	Rs.	A.
Quarrying stone, including all materials and labour, per 100 cubic feet	3	8
Carriage of stone (5 miles lead), eight carts, each containing 12½ cubic feet, at R.1¼ per cart	10	0
Dressing (one man will dress 12 cubic feet per day at R.1 2a. per day)	9	9
Setting (one man will set 20 cubic feet per day at R.1 2a. per day)	5	10
Nowgunnies, coolies, and native maistry	6	0
Sand, lime and water	4	0
Total for 100 cubic feet of block-in-course masonry	38	11

Or equal to £1 0*s*. 11*d*. per cubic yard.

A superior class of coursed rubble per 100 cubic feet, exclusive of European superintendence and plant:—

	Rs.	A.
Quarrying stone, including all materials and labour, per 100 cubic feet	2	10
Carriage of stone (5 miles lead), seven carts, containing 12½ cubic feet, at R.1 2a. per cart	7	14
Dressing (one man will dress 33 cubic feet per day at R.1 per day)	3	0
Setting (one man will set 33 cubic feet per day at R.1 per day)	3	0
Coolies and native maistry	2	0
Sand, lime and water	4	0
Total for 100 cubic feet of superior class of rubble	22	8

Or equal to 10*s*. 6½*d*. per cubic yard.

Common rubble (chips with headers) per 100 cubic feet, exclusive of European supervision and plant :—

	Rs.	A.
Stone at quarry	0	5
Carriage (5 miles lead), five carts at R.1 2a. each . .	5	10
Labour of mason (one mason at R.1 will prepare and set 40 cubic feet per day	2	8
Coolies and maistree	2	0
Sand, chunam and water	4	0
Total for 100 cubic feet of common rubble . .	14	7

Or equal to 7*s.* 9½*d.* per cubic yard.

Daily wages of workmen in Karachi :—

		Rs.	A.		Rs.	A.
Carpenter	from	0	12	to	1	4
Mason	,,	0	10	,,	1	4
Smith	,,	0	12	,,	1	2
Hammerman	,,	0	8	,,	0	10
Coolies	,,	0	4	,,	0	6

Cost of building materials :—

	Rs.	A.	
Lime	3	8	per karar of 33½ cubic feet.
Moulmein teak . . .	5	0	per cubic foot.
Jungle wood	2	8	,,

APPENDIX IV.

ANALYSIS of WATER SUPPLIED to KARACHI, and the TROOPS in the GARRISON, PRIOR to the OPENING of the KARACHI WATER WORKS in 1882.

Dates of Collection.	Source.	Grains per Gallon.					Permanent hardness.	Parts per Million.	
		Total Solids.	Chlorine.	Sulphuric Acid.	Lime.	Magnesia.		Free Ammonia.	Albumenoid Ammonia.
1 Oct. 1875	Filter tank, Napier Barracks	49·91	16·10	..	..	..	..	0·26	0·18
5 Nov. 1875	Wells in front of R.A. Barracks	139·30	54·37	15·77	6·09	26·04	..	0·02	0·18

APPENDIX V.

ANALYSIS of WATER TAKEN from the WELLS on the BANKS of the MALIR RIVER, from which the CITY of KARACHI is NOW SUPPLIED with WATER.

Date of Collection.	Source.	Grains per Gallon.					Permanent hardness.	Parts per Million.	
		Total Solids.	Chlorine.	Sulphuric Acid.	Lime.	Magnesia.		Free Ammonia.	Albumenoid Ammonia.
11 Oct. 1875	Malir River wells	24·36	4·06	2·40	3·45	2·32	5°·2	0·04	0·08

The water was collected at a depth of 18 feet below the surface of the ground, or 10 feet below the bed of the river.

APPENDIX VI.

TABLE of RAINFALL and TEMPERATURE at KARACHI.

Year.	Rainfall.[1]	Year.	Rainfall.[1]	Month.	Mean[2] Rainfall 25 years.	Mean[2] Temperature 8 years.
	Inches.		Inches.		Inches.	
1856	2·14	1870	4·65	January . . .	0·43	65°·3
1857	5·70	1871	0·12	February . . .	0·23	68·2
1858	5·90	1872	7·60	March	0·25	76·6
1859	6·20	1873	2·50	April	0·24	80·4
1860	2·80	1874	7·93	May	0·12	85·4
1861	5·71	1875	8·13	June	0·01	86·5
1862	4·86	1876	17·88	July	1·90	83·9
1863	14·50	1877	2·82	August . . .	2·99	82·0
1864	6·28	1878	25·50	September . .	0·92	82·1
1865	7·06	1879	2·02	October . . .	0·00	79·5
1866	13·73	1880	3·89	November . .	0·10	72·4
1867	2·41	1881	11·03	December . . .	0·18	67·3
1868	2·86	1882	10·18			
1869	28·45	1883	6·63	Year . .	7·37	77·5

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[1] Minutes of Proceedings Inst. C.E., vol. xxvii. p. 250.

[2] Report on the Meteorology of India in 1882. By Henry F. Blanford, Meteorological Reporter to the Government of India.

(*Paper No. 2081.*)

"The Design and Construction of Railway Rolling-Stock in Italy."

By S. Fadda, Chief of the Department for Preliminary Studies of Rolling-Stock of the Upper Italy Railway.

Translated and abstracted by W. H. Thelwall, M. Inst. C.E.

Introduction—Historical Summary.

At the time when railways were invented and brought into practical use by the genius and energy of England, and their construction upon the continent of Europe was commencing, Italy was, unfortunately, divided into a number of small States, each of which maintained a policy of isolation from its neighbours. As however the force of civilization gradually overcame that of despotism, railways were introduced into the peninsula and became an important factor in achieving its political union.

The first line built in Italy, that between Naples and Portici (a distance of 5 miles), was opened in October 1838, and from that date railways began to spread over the country. The first which offered serious engineering difficulties was that from Genoa to Turin, crossing the Appenines. The late Mr. Brunel was consulted in reference to this work and laid down a line with a gradient of 1 in 28½, which was steeper than anything which had previously been attempted. Upon the recommendation of the Chev. H. Maus, who succeeded Mr. Brunel, experiments were made with a view to working this gradient by means of a system of hydropneumatic propulsion proposed by Messrs. Sommeiller, Grattoni, and Grandis. The result was not satisfactory, but the system was afterwards successfully employed in the construction of the Frejus tunnel upon the Turin-Modane line.

It was not till the year 1859 that railways were opened in the Duchies of Parma and Modena and in the Papal States.

In 1861, when the Kingdom of Italy was proclaimed, there were 2,189 kilometres (1,360 miles) of railways open in different parts of the country. A considerable portion of this mileage had been laid down in detached lengths without any thought of their ultimate connection into a combined system. The Government at once undertook the difficult task of completing and connecting the various lines in a systematic manner. On the 14th of March, 1865, a law for regulating the construction and working of railways was passed, in which the principle of private ownership and working was established, except for lines constructed by the State, and concessions for working these latter were granted to private companies.

The lines were divided into four main groups, the Upper Italian railways (*le Ferrovie dell' Alta Italia*), the Roman railways (*le Ferrovie Romane*), the Southern railways (*le Strade ferrate Meridionale*), and the Calabro-Sicilian railways (*le Ferrovie Calabro-Sicule*).

From this time up to 1876 several important mountain lines were constructed. In that year the Upper Italian lines became the property of the State, as did also the Roman lines later on. Concessions for working the whole of the lines and for the construction of others have recently been granted to three companies, namely, the "Società Italiana per le Strade ferrate del Mediterraneo," the "Società Italiana per le Strade ferrate dell' Adriatico," and a company for working the Sicilian lines. The lengths of lines assigned to the three companies are: to the first, 4,106 kilometres open, and 1,968 under construction; to the second, 3,982 kilometres open, and 1,881 under construction; to the third, 599 kilometres open, and 533 under construction. At the present time there are altogether in Italy, including lines not assigned to either of the above companies, 9,182 kilometres (5,705 miles) of railway open, and 3,887 kilometres (2,415 miles) under construction; making a total of 13,069 kilometres (8,120 miles), which will be increased to 15,000 kilometres (9,320 miles) when the lines voted by Parliament this year (1885) are constructed. There are in addition 1,585 kilometres (985 miles) of tramway in the country.

The Table on page 353 gives the lengths of the various lines, and the quantities of rolling stock in use on each.

The conditions under which many of these lines are worked present considerable difficulties, and the object of the present Paper is to explain the means adopted, and also the progress made from time to time in Italian industries for supplying the demands of the traffic. It is not intended to discuss the construction of railways, for which Italian engineers have obtained, and with reason, a European reputation; but to treat of the rolling-stock under the heads of

——	Kilometres Open.	Miles Open.	Wagons.	Carriages.	Engines.
Upper Italian railways .	3,729	2,317	14,407	2,439	902
Roman ,, .	1,677	1,042	4,682	1,177	299
Southern ,, .	1,574	978	4,190	765	282
Calabro Sicilian ,, .	1,332	828	2,218	540	197
Sardinian ,, .	388	241	406	96	28
Turin-Lanzo ,, .	32	20	93	35	5
Western Sicilian ,, .	194	121	162	60	18
Venetian ,, .	154	96	156	102	21
Milan-Saronno Erba railways	67	41	52	67	14
Settimo-Rivarolo railways	23	14	4	10	3
	9,170	5,698	26,370	5,291	1,769
Proportions per kilometre	..	..	3·088	0·564	0·19
Tramways	1,585	985	..	1,713	384
Lines in construction . .	3,887	2,415	..	..	..

Locomotives, Carriages and Wagons; a brief account of the principal works which have been established in Italy for supplying railway plant will follow.

THE LOCOMOTIVE UPON ITALIAN RAILWAYS.

Up to the year 1852 uncoupled engines with an adhesive weight of 5 tons were sufficient for the traffic, but in that year 10-ton engines were introduced. In 1855 the adhesive weight was increased to 12½ tons with heating-surface of 88 square metres (946 square feet), and tractive force of 1,225 kilograms. With the lengthening of the lines and the great increase of traffic, while the excellent uncoupled engines, furnished for the most part by the celebrated English firms of Stephenson, Sharp, and Beyer Peacock Co.,[1] still served for quick trains, they were quite insufficient for heavy goods trains, and engines with four wheels coupled were introduced to secure greater adhesive power. As, however, mountain lines were constructed, some, as before stated, with gradients of 1 in 30 and less, still more powerful machines were required, and very heavy engines with six or eight wheels coupled were adopted. In order better to examine the various types now working in Italy, they will be divided into three classes:—

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[1] A great number of these engines, dating from 1853, are still in use, and giving the greatest satisfaction to the traffic department.

a. Passenger engines.
b. Goods engines.
c. Engines for mountain lines.

(*a*) *Engines for Passenger Trains.*—The gradients upon many of the Italian lines are so steep that very powerful engines are required even for passenger trains; in fact nearly all the engines may be so used; but in speaking of lines in flat districts the passenger engines may be divided into three classes.

1. Engines with uncoupled wheels.
2. Engines with four wheels coupled.
3. Engines with six wheels coupled.

1. *Engines with uncoupled wheels.*—Nearly all these engines are of the English type, and built by the following firms, viz.: R. Stephenson and Co.; Canada Works, Birkenhead; Sharp, Stewart, and Co.; Beyer, Peacock, and Co.; Parent, Schaken, Caillet, and Co.; and J. F. Cail and Co.; and the National Establishment of Pietrarsa. The speed to which they are adapted is 30 miles an hour. Where higher speed and greater tractive force are required it becomes necessary to change the type of the fire-box. Plate 7, Fig. 1, shows one of these modified engines built by Messrs. Sharp, Stewart, and Co. It was originally intended for burning coke, and the area of the fire-grate was therefore small (15 square feet), while its depth was comparatively great. The air having to traverse a thick stratum of coke, a strong blast was necessary, and consequently the heated gases passed out of the chimney at a high temperature (from 750° to 850° Fahrenheit), and lost a part of their useful effect. As is the case in all internally-fired boilers, the useful effect of the fuel increases with the amount of radiant heat set free during combustion, for the reason that this heat is immediately absorbed by the walls of the grate and transmitted to the water. In coke the radiant heat amounts to 67 per cent. of the total, while in *litantrace*[1] it is scarcely 55 per cent. This, combined with the fact of its giving less smoke, was the reason for the adoption of the former during the early years of railway working, although the cost was greater. However, as the price of coke increased with the demand, especially in Italy where it was always high, the substitution of *litantrace* began to be seriously considered.

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[1] The translator has not been able to ascertain whether there is an English equivalent for "litantrace." It appears to resemble *cannel* coal, but it is doubtful whether the two terms are synonymous.—W. H. T.

With this view the grate area was gradually increased, and the thickness of fuel reduced. The production of steam was thus rendered cheaper and the evaporative power increased in proportion to the greater power required from the engines. There was also another circumstance which caused a modification of the fire-boxes. In the transport of coal from England and France a large amount of dust was produced, which had to be formed into blocks at a considerable expense. It therefore was desirable to find some way of burning this dust and small coal without previous manipulation. The first proposal in this direction was that of the Belgian engineer Belpaire, with fire-boxes 9 feet in length. Similar fire-boxes were adopted by the Engineers of the Northern Railway of France for similar reasons. Italy soon followed the example, and as the results of the first experiments were very satisfactory, the principle was adopted in all the new, and applied to many of the existing engines. This accounts for the fact that in nearly all the engines hereafter referred to the long type of fire-box will be found. It is considered in Italy that with long grates capable of burning small (and therefore cheap) coal the following advantages can be obtained :

1st. Economy in fuel.

2nd. Greater evaporative power, and hence the possibility of increasing within certain limits the power of the engines.

3rd. Increased useful effect from the fuel, as the heated gases pass off at a lower temperature.

4th. Other circumstances being equal, increased useful effect from the steam in the cylinders, because the force necessary for drawing the air through the fire being diminished, the blast may be less powerful, and hence the back pressure in the cylinder may be lower.

True these advantages are accompanied by sundry inconveniences, such as, greater cost of the fire-box, increased weight of engine and cost of maintenance, and more work for the enginemen. The experience of ten years, especially on the Upper Italian railways, has, however, shown that the advantages outweigh the inconveniences.

Turning now to the modified Sharp engine, Plate 7, Fig. 1, the differences between the new and the old form are shown below.

The unaltered dimensions are :—

Diameter of driving-wheels . .	1·975 metre	(6 feet 6 inches).
,, cylinders . . .	0·406 ,,	(16 inches).
Stroke	0·508 ,,	(20 ,,).

The variable dimensions are :—

	Old Type.	New Type.
Surface of fire-grate .	1·40 sq. m. (15 sq. ft.)	2·20 sq. m. (23½ sq. ft.)
Direct heating surface .	8·20 ,, (88¼ ,,)	9·50 ,, (150½ ,,).
Heating surface of tubes	87·90 ,, (946 ,,)	80·00 ,, (861 ,,).
Adhesive weight . .	12,500 kilo. (12·3 tons)	13,000 kilo. (12¾ tons).
Tractive force at 37 miles per hour	1,220 ,, (2,688 lbs.)	1,350 ,, (2,975 lbs.).
Working-pressure	7 atmospheres	7 atmospheres.

This was the maximum improvement which could be obtained, keeping the adhesive weight at 13,000 kilograms, without increasing the steam-pressure.

Engines with four wheels coupled.—The increasing weight of fast trains has rendered necessary even in England, where the amount of traffic admits of a great number of express trains, the introduction of heavy engines, which, whilst maintaining a speed of from 35 to 45 miles an hour, are powerful enough to draw heavy loads. In Italy, where the traffic is not very great and where the trains are fewer and heavier, the need for this class of engines was even greater, especially when, as on many lines, mixed passenger and goods trains are run. It was to meet this want that engines with four coupled wheels were introduced. Since the year 1862 the managers of Italian railways have been giving up inside cylinders, and in 1875 the authorities of all the principal lines agreed upon the point on account of the frequent breakage of the axles. A number of different types of engines were introduced, but the one principally adopted up to 1870 was that known as the Médoc engine. This engine has outside cylinders 420 millimetres (16½ inches) in diameter, 560 millimetres (22 inches) length of stroke, adhesive weight, 23,100 kilograms (22¾ tons); working-pressure, 9 effective atmospheres; fire-box heating-surface, 7 square metres (75 square feet); heating surface of tubes, 101·6 square metres (1,094 square feet); tractive force at the normal velocity of 45 kilometres (28 miles), 2,060 kilograms (4,540 lbs.). These and other similar engines answered well for ordinary passenger and mixed trains.

As traffic continued to develop, the express trains had to be increased in length and weight (but could not be run more frequently owing to the necessities of the train-service). Towards the end of 1872, it was found necessary to have engines with four wheels coupled, capable of running 60 kilometres (37 miles) per hour. The type adopted for this purpose by the Upper Italian lines is shown in Plate 7, Fig. 2, and this was the point of departure in the reform of rolling-stock. Up to this time everything that concerned the design of the engines was left to the builders, and very few Italian engineers troubled themselves about their construction.

To the suggestion of Mr. Frescot, the present Chief of the railway-material department, is due the establishment by Director Amilhau, of a school of railway mechanics, which has produced the best results; and at the present time all the designs of the rolling-stock are made in the country, with the results shown at the exhibitions of Vienna, Paris, Milan, and Turin. The engine shown in Plate 7, Fig. 2, was built by the "Société Alsacienne de Constructions Mécaniques," of Mulhausen. The principal dimensions of this engine are—

Diameter of driving-wheels . . .	2·020 metres (6 ft. 7½ in.).
,, cylinders	0·430 ,, (17 inches).
Stroke	0·620 ,, (24½ ,,).
Adhesive weight	25,100 kilograms (24·7 tons).
Direct heating-surface	9·00 square metres (97 sq. ft.).
Heating-surface of tubes	94·50 ,, ,, (1,017 ,,).
,, ,, fire-grate . . .	2·20 ,, ,, (23¾ ,,).
Steam-pressure	9 atmospheres (effective).
Tractive force at the normal velocity	2,050 kilograms (4,520 lbs.).

These engines proved most successful, and were adopted for a variety of work, for omnibus and mixed trains. There not being enough of them, however, and there being more uncoupled engines than were required, the directors of the Upper Italian railways determined to convert some of them into coupled engines. The type selected was one of Stephenson's shown, as altered, in Plate 7, Fig. 3. The comparative dimensions are as follow:—

	As Built.	As Altered.
Diameter of driving-wheels	1·882 m. (6 ft. 2 in.)	1·882 m. (6 ft. 2 in.).
Diameter of cylinders .	0·406 ,, (16 in.)	0·406 ,, (16 in.).
Stroke	0·508 ,, (20 ,,)	0·508 ,, (20 ,,).
Adhesive weight . .	12,400 kilo. (12·2 tons)	26,000 kilo. (25½ tons).
Heating-surface (direct)	7·43 sq. m. (80 sq. ft.)	9·65 sq. m. (104 sq. ft.).
,, ,, of tubes	92 ,, (990 ,,)	92 ,, (990 ,,).
Surface of fire-grate . .	1·27 ,, (14 ,,)	2·30 ,, (25 ,,).
Steam-pressure . . .	7 effective atmospheres	9 effective atmospheres.
Tractive force at the normal velocity of 45 kilometres (28 miles) per hour	1,380 kilograms	1,640 kilograms.

The alterations were commenced in 1877, and are still carried on in the workshops of the Upper Italian railways at Verona.

About the same time a type of bogie express-engine (Plate 7, Fig. 4), was designed in the shops of the Upper Italian railways for use upon lines with sharp curves. The principal dimensions are:—

Diameter of driving-wheels . . .	1·820 metre (6 ft. 7½ in.).
,, cylinders	0·430 ,, (17 inches).
Stroke	0·560 ,, (22 ,,).
Adhesive weight	26,400 kilograms (26 tons).
Heating-surface (fire-box)	8·70 square metres (94 sq. ft.).
,, ,, of tubes	88·60 ,, ,, (954 ,,).
Surface of fire-grate	2·15 ,, ,, (23 ,,).
Steam-pressure	10 atmospheres (effective).
Tractive force at the normal velocity	1,800 kilograms (2,380 lbs.).

Both this class and the altered engines have answered the purposes for which they were intended extremely well.

In the latter engines an invention of Mr. O. Chiazzari was introduced for heating the feed-water on its way to the boiler by means of the waste steam from the cylinders. A gold medal was awarded for this invention at the Paris Exhibition of 1878.

As trade between Genoa and Alessandria increased, it was found that the train-service over the Giovi Pass, with its incline of 1 in 28½, was so difficult and slow that not more than 900 wagons per day could pass over the line even with the heaviest type of engine. It was therefore decided to construct a new line called the "Succursale dei Giovi," which, starting from Sampierdarena, runs to the station of Ronco, with gradients not exceeding 16 per 1,000. As the passenger trains from Rome on to the Upper Italian Railways, and to the St. Gothard and Mont Cenis pass over this line, it was necessary to design engines which, starting from Genoa, could draw passenger trains to Alessandria at 60 kilometres (37 miles) per hour on the plains, and 45 kilometres (28 miles) on a gradient of 1 in 62. Plate 7, Fig. 5, shows the engine designed and constructed in the shops of the Upper Italian railways and exhibited in the National Exhibition of Turin in 1884. This engine has six wheels coupled, and a bogie-truck so arranged that it can take a lateral displacement so as to run more easily round curves. This is effected by suspending it from four tie-rods in a manner similar to an engine of the Taff Vale Railway.[1]

The cylinders are put between the bogie-truck and the leading driving-wheels, in order to place the centre of motion near the centre of gravity, and thus minimize the rocking of the engine when running. Chiazzari's automatic lubricators are used for the cylinders.[1] The wheels are of wrought iron, and made in the Turin workshops. They were the first so constructed in Italy.

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[1] "Engineering," 1885, p. 266.

The boilers differ in some respects from those previously introduced. In order to develop the power and high speed required, a large and rapid production of steam was necessary. For this purpose, a large fire-grate was adopted with an area of 2·200 square metres (23⅝ square feet) which, when burning only 400 kilograms of coal per square metre per hour (82 lbs. per square foot), produces the required quantity of steam. To obtain combustion in so large a grate, an intimate admixture of the air and heated gases must take place, which is effected by enlarging the space for combustion, and causing rapid changes of direction in the air currents. This last condition can be obtained by adopting the Nepilly fire-box: but as this does not give sufficient space, it was decided to prolong the fire-box within the cylindrical parts of the boiler in a method similar to that adopted by McConnell since 1853, and followed by Baldwin and others. In order to obtain the necessary deviation in the currents, a step, made of fire-bricks, is inserted where the fire-box is prolonged into the boiler. The advantages of this system are:—

1st. The conditions of combustion are improved, and the smoke is diminished.

2nd. The tube-plate is removed from direct contact with the flames, and will consequently last longer.

3rd. The tubes are preserved from the violent action of the flames, and being shorter, they are subject to less expansion and contraction, so that they are less frequently broken.

The boiler, when the water is only 10 centimetres (4 inches) above the fire-box, contains 5 cubic metres (177 cubic feet). The working pressure is 10 atmospheres (effective). With so large a volume, sufficient heat can be stored up to maintain a nearly constant pressure, notwithstanding the irregularities of the road. The diameter of the exhaust-pipe is constant throughout its length, as is shown in Plate 7, Fig. 5. The blast is regulated by a damper at the bottom of the smoke-box, under the control of the driver. By this means the smoke-box is easily kept clean. The exhaust-pipe terminates in three concentric cones, which have the effect of producing a regular passage of heated air through all the tubes.

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[1] For a description of this appliance—which answers well—see "Sul razionale Ungimento delle macchine a vapore e sui modi di ottenerlo," by Orazio Chiazzari. Civelli. Torino. 1884.

The principal dimensions of the engine are as follow :—

Diameter of driving-wheels . . .	1·675 metre (5 ft. 6 in.).
,, cylinders	0·475 ,, (18¾ inches).
Stroke	0·620 ,, (24½ ,,).
Heating-surface (fire-box). . . .	15·200 square metres (164 sq. ft.).
,, ,, of tubes	108·800 ,, ,, (1,171 ,,).
Area of fire-grate	2·200 ,, ,, (24 ,,).
Working-pressure	9 atmospheres (effective).
Adhesive weight	39,034 kilograms (38·4 tons).
Tractive force at 60 kilometres (37 miles) per hour	2,585 ,, (5,700 lbs.).

Engines for Economical Train-Service.—There are some lines which, particularly at certain seasons of the year, owing to the limited number of passengers, cannot be worked conveniently by large engines; and upon some of the principal lines short trains at frequent intervals are required. Numerous designs and experiments have been tried with a view to providing engines suitable for traffic of this description. Belpaire's system, in which the engine formed a part of one of the carriages, was tried, but did not answer. Next, Krauss's engine, having an adhesive weight of 15,000 kilograms (15 tons) was tried. This answered well enough for light trains upon lines having easy gradients, but as it was too light for most of the lines, other types were introduced; that shown in Plate 7, Fig. 6, weighing 21,000 kilograms, being adopted upon the Upper Italian railways. This is a very useful class of engine. A number of tramway locomotives are also employed, but do not come within the scope of the present Paper.

(*b*) *Goods Engines.*—To a great extent the train-service is a mixed one, of passengers and goods combined, and the engines already described are used. Where engines are used for goods only, the type is that shown in Plate 7, Fig. 7, with six coupled wheels, outside cylinders and internal machinery. This type, and others closely resembling it, are used in large numbers on all the Italian lines. The principal dimensions are :—

Diameter of wheels	1·320 metre (4 ft. 4 in.).
,, cylinders	0·450 ,, (17¾ inches).
Stroke	0·650 ,, (25⅝ ,,).
Heating-surface (fire-box)	8·50 square metres (91 sq. ft.).
,, ,, of tubes	116·20 ,, ,, (1,250 ,,).
Area of grate	1·60 ,, ,, (17 ,,).
Adhesive weight	39,000 kilograms (38·4 tons).
Tractive force at 30 kilometres (19 miles) per hour	3,400 ,, (7,496 lbs.).

These engines are adopted for passenger trains upon certain mountain-lines, while mountain-engines are used upon some lines for goods-trains.

(*c*) *Mountain-Engines.*—As already stated, the problem of working trains on heavy gradients presented itself during the first years of railway construction in Italy. The first method of working the Giovi incline was to couple two four-wheeled tank-engines together, back to back, so that they could be driven by one man. This plan, proposed by the Italian engineer Ruva, was approved by Stephenson. The principal dimensions of the engines were :—

Diameter of wheels (four-coupled) .	1·070 metre (3 ft. 6 in.).
,, cylinders	0·335 ,, (13⅕ inches).
Stroke	0·559 ,, (22 ,,).
Heating-surface (fire-box) . . .	6·40 square metres (69 sq. ft.).
,, ,, of tubes	59·00 ,, ,, (635 ,,).
Working-pressure	7 atmospheres (effective).
Adhesive weight	27,000 kilograms (26½ tons).
Tractive force at 20 kilometres (12½ miles) per hour	2,500 ,, (5,511 lbs.).

These engines drew trains weighing (in addition to the engines) from 70 to 90 tons, at speeds varying from 15½ to 10 miles per hour; but on the traffic increasing, engines with six wheels coupled, weighing 36,000 kilograms (35½ tons) were used in a similar way. By the year 1860 these also proved insufficient, and special engines, having eight wheels coupled and tenders, were designed and built of two slightly different types. In one type, the tenders had six wheels, in the other, four; in both, the fore-wheels of the tender were arranged to carry part of the weight of the engine. These engines were designed by Beugnot, and, though triumphs of mechanical skill, had several practical defects. With the idea of enabling them to run round curves of 100-metres (328 feet) radius, the leading and trailing wheels were arranged to turn round a central pivot, and this involved such a complicated system of springs, pivots, &c., that cleaning was a very difficult process, and the cost of maintenance and the difficulty of erection were very great. Moreover, these complications were not essential, as the sharpest curves on the lines were of 300 metres (984 feet) radius. They had a further defect, that the diameter of the cylinders was too large, the result being that the force developed in them exceeded the adhesive weight, and caused the wheels to slip. The engines were, therefore, after a time remodelled, and this last defect was remedied by putting a lining of cast-iron inside the cylinders.

When, however, in 1872 it became necessary to increase the stock on the mountain lines, a different type was introduced, which was suitable for the gradients on these lines, and combined the greatest simplicity with facility of maintenance. The type adopted was that furnished by G. Sigl of Wiener-Neustadt, and working very successfully on the Semmering Pass, the engines being designed at the Turin shops of the Upper Italian railways (Plate 7, Fig. 8). In these engines the whole of the motion is placed outside, rendering the work of cleaning and putting together very easy. The axle-boxes of the trailing wheels have a lateral play of 20 millimetres ($\frac{3}{4}$ inch). The weight, 52,000 kilograms (51 tons), is equally distributed over the four axles. The following Table shows the superiority of the Sigl over the two types of Beugnot engines :—

——	Beugnot's Engine.				Sigl's Engine.	
	Type 1.		Type 2.			
	Metre.	Feet.	Metre.	Feet.	Metre.	Feet.
Diameter of coupled driving-wheels	1·210=	4	1·210=	4	1·210=	4
		Inches.		Inches.		Inches.
Diameter of cylinders .	0·600=	$23\frac{5}{8}$	0·600=	$23\frac{5}{8}$	0·530=	$20\frac{7}{8}$
Stroke	0·610=	24	0·610=	24	0·610=	24
	Sq. Metres.	Sq. Feet.	Sq. Metres.	Sq. Feet.	Sq. Metres.	Sq. Feet.
Heating-surface (fire-box)	9·98 =	107	10·70 =	115	10·70 =	115
,, ,, of tubes .	160·00 =	1,722	166·00 =	1,786	158·00 =	1,700
Area of fire-grate . . .	2·59 =	28	2·46 =	26·47	2·15 =	23·14
	Effective Atmospheres.		Effective Atmospheres.		Effective Atmospheres.	
Working pressure . . .	8		8		9	
	Kilograms.	Lbs.	Kilograms.	Lbs.	Kilograms.	Lbs.
Normal tractive force. .	6,400 =	14,108	6,400 =	14,108	6,550 =	14,439
	Tons.		Tons.		Tons.	
Load drawn at 25 kilometres ($15\frac{1}{2}$ miles) per hour up gradient of 35 per thousand . . .	118		118		133	
	Kilograms.	Tons.	Kilograms.	Tons.	Kilograms.	Tons.
Adhesive weight . . .	49,600=	48·8	50,000=	49·2	52,100=	51·3

The usual practice on the Giovi incline is to have an engine at each end of the train; but for very heavy trains, two engines are placed in front and one engine behind. Sigl's engines are also used for heavy traffic on lines in the plains.

To avoid the inconvenience of an accumulation of sand in tunnels, Riggenbach's system of wetting the rails and tires with hot water to increase adhesion has been adopted with great success for some years. This answers the same purpose, and does not wear the rails and tires so much as sand.

On the mountain lines of the Southern railways two types of engines have been adopted, with eight wheels coupled, and external cylinders and motion; one (by Borsig) being a tank-engine, the other having a tender:—

	Borsig's Engine. 8 wheels coupled.		Tender-Engine. 8 wheels coupled.	
Diameter of wheels	1·270 m.	(4 ft. 2 in.)	1·100 m.	(3 ft. 7 in.).
,, cylinders	0·520 ,,	($20\frac{1}{2}$ in.)	0·430 ,,	(17 in.).
Stroke	0·660 ,,	(26 ,,)	0·520 ,,	($20\frac{1}{2}$,,).
Total heating-surface	146·5 sq. m.	(1,577 sq. ft.)	93·0 sq. m.	(1,000 sq. ft.).
Area of fire-grate	2·1 ,,	($22\frac{1}{2}$,,)	1·52 ,,	($16\frac{1}{3}$,,).
Weight of engine running	48,000 kilo.	($47\frac{1}{4}$ tons)	39,900 kilo.	($39\frac{1}{4}$ tons).

MATERIALS AND METHODS OF CONSTRUCTION EMPLOYED IN ITALIAN ENGINES.

Boilers.—The materials generally adopted for these is iron, of which two qualities are used. (*a*) That for the cylindrical body must be capable of resisting a tensile strain of 34 kilograms per square millimetre ($21\frac{1}{2}$ tons per square inch), with an extension of 15 per cent. in the direction of the fibre, and 10 per cent. across it. (*b*) For the fire-box and tube-plate of the smoke-box, charcoal-iron is employed, capable of resisting a tensile strain of 36 kilograms per square millimetre (22·86 tons per square inch), with extensions of 20 and 16 per cent. in the direction of and across the fibre respectively. The rivets are of Swiss charcoal-iron, capable of resisting a tensile strain of 38 kilograms per square millimetre (24·13 tons per square inch), with extension of 20 per cent. Plates of steel and of so-called homogeneous iron (*flusseisen*) are absolutely forbidden for locomotive-boilers, except in the case of certain tramway-engines.

Fire-boxes.—These are always made of copper, as are also the stay-bolts. The resistance has to be at least 24 kilograms per square millimetre ($15\frac{1}{4}$ tons per square inch), with elongation of 30 per cent. in both directions. Steel has not been tried for fire-boxes except in the case of small tramway-engines, in which the results were very bad.

Boiler-tubes.—These are generally made of brass, consisting of 70 per cent. of copper and 30 per cent. of zinc, with a permitted deviation of $\frac{1}{2}$ per cent. more or less. They vary in diameter from 45 to 52 millimetres (1·77 inch to 2·05 inches), and in thickness from 0·098 to 0·118 inch. The conditions specified for the tubes are:—

(1) Resistance to an internal pressure of 25 effective atmospheres applied by the hydraulic press, without showing any signs of leaking.

(2) To be capable of resisting the application cold of a ferrule of 0·41 inch without cracking or splitting.

(3) One tube taken at random out of every two hundred is to be filled with tar and bent to a curve having a chord of 20 inches and versed sine of $2\frac{1}{3}$ inches without cracking, splitting, or failing in any way.

(4) To be of perfectly cylindrical form both externally and internally, of uniform thickness, and of the exact dimensions shown in the drawings.

Recently tubes of iron and steel have been tried, but there has not been time to decide as to their merits, especially seeing that upon other European lines iron tubes, after having been used for some years, are now being given up in favour of brass.

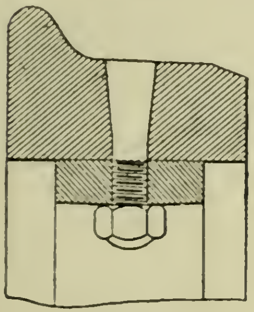
Two kinds of safety-valves are applied to each boiler, one the ordinary balance-valve, and the other inaccessible to the driver. For the latter the Ramsbottom-Webb valve is generally used, and in some cases Wilson's. Each boiler is tested once a year both cold and under steam, by the Government Commissioner. Every five years the tubes are taken out, and the boiler undergoes an internal inspection. The testing is repeated every time the engine is sent out of the repairing shops. The normal working pressure in railway engines is from 8 to 9 effective atmospheres, and never exceeds 10. On tramways and light railways, pressures of 12 and 15 atmospheres are used.

Wheels and axles.—The wheels, both of engines and tenders, are all of wrought iron, the counterweights, when there are any, being made in the same piece with the wheel. The tires of the engines are of crucible steel, and for the tenders, of Bessemer or Siemens steel.

(*a*) *Tires of Crucible Steel.*—A tire, when cold, is submitted to weight of 1 ton, falling through a height of 10 feet, so as to develop a force of 10 foot-tons. After five blows, the tire, though flattened, must not show any cracks or other defects. Rupture must not take place till the flattening has reached one-fifth of the original internal diameter of the tire. The section of rupture must exhibit a firm, compact, and homogeneous grain. Bars are cut out of the fractured tire which, on being tested, must give the following results:—resistance to tensile strain 70 kilograms per square millimetre (43·6 tons per square inch); extension, 15 per cent.

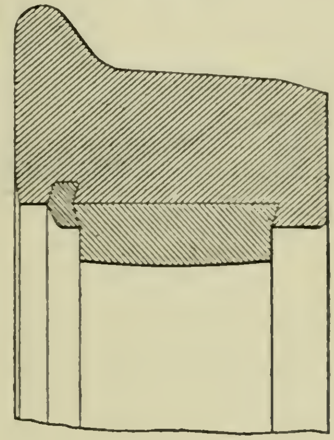
(*b*) *Tires of Bessemer and Siemens-Steel.*—A test similar to that for crucible-steel tires is to be applied; but in this case, the force is to be 4·85 foot-tons, the weight being from 1,700 to 1,900 lbs. The tire at each blow must not bend more than $\frac{1}{2}$ inch, and it must not break till the flattening has reached three-fifths of the original diameter. The section of rupture must be of fine grain, compact, very homogeneous, free from air-holes and other defects, and the tenacity of the steel must be apparent from the absence of crystalline structure. Bars cut from the broken tire in the direction of the grain must give the following results:—resistance not less than $31\frac{3}{4}$ tons per square inch, with elongation 22 per cent.

FIG. 1.



Scale ½.

FIG. 2.



Scale ½.

Two methods of fastening on the tires are shown in Figs. 1 and 2. The axles of all the wheels, whether straight or cranked, are made of the best quality of Siemens steel, very soft, tempered with difficulty, and giving a tensile resistance of $31\frac{3}{4}$ tons per square inch of original section, with elongation 25 per cent.

The frames are of wrought-iron, having a tensile resistance of 20 tons per square inch, with elongation 9 per cent. with the grain, and 6 per cent. across it.

Moving parts.—The cylinders are always of cast-iron, and are provided with automatic lubricators. The valve-slides are usually of ordinary gun-metal, in some engines they are of cast-iron. Phosphor-bronze has been tried, but it is not thought superior to gun-metal. For the piston- and valve-rods, Dallot-Rognetta's metallic stuffing is used. All parts of the motion are of Siemens steel, except the links which are of hammered iron. The piston-rods, guide-bars, and all the pivots and keys of the motion, are of crucible steel. The cross-heads are of wrought-iron tempered.

The tenders present no special peculiarities. For the most part they have three axles. All are fitted with hand-brakes.

The following Table shows the number of locomotives placed upon the principal lines between the dates given, and also the countries from which they were obtained. The list does not include the engines upon lines of little importance or on tramways:—

——	Germany, Austria, Switzerland.	England.	France and Belgium.	Italy.
From 1847 to 1865	36	245	334	87
,, 1866 ,, 1875	80	40	267	82
,, 1876 ,, 1885	322	36	6	171[1]
	438	321	607	340

Carriages on Italian Railways.

The type originally adopted on nearly all the lines was that known as the English, divided into separate compartments; upon a few, however, the American type, consisting of long carriages with end-platforms and central passage was adopted. The American carriages have, however, all been withdrawn from traffic, and are serving the purpose of temporary offices at some railway station.

Trains of first, second, and third class carriages are run upon the greater part of the lines, and in some parts of Southern Italy a fourth class is added. With the increase in length of lines and consequent prolonged journeys, improvements were gradually introduced into the carriages.

First-class Carriages and Saloons.—Plate 8, Fig. 9, shows one of the earliest carriages, constructed in 1848, and still in use. Each compartment will hold eight passengers. The width inside is 2·630 metres (8 feet 7½ inches), which, after deducting the arms, allows 1 foot 11 inches for each passenger. The dead weight per seat is 495 lbs., and the length of each compartment 5 feet 11 inches.

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[1] This number (171) includes certain engines which have been ordered, but are not yet completed.

In carriages of more recent construction, four compartments for eight passengers each are carried upon two axles, the internal width is increased to 8 feet 3 inches, giving, including the arms, 2 feet 0¾ inch for each passenger. The dead weight per seat is 771 lbs., the length of a compartment 6 feet 10 inches, and the height is increased by 8 inches.

As traffic developed, and the length of journeys increased, there began to be a demand for sleeping accommodation. This was met at first by the introduction of sleeping coupés. Carriages were built similar to those last described, with the exception that one of the end compartments was turned into a coupé, in which three beds were provided. These, however, were inconvenient, having no lavatory accommodation. To meet this want, the carriage shown in Plate 8, Fig. 10, was designed, with two central compartments A, A′, each of which contains three beds *s*, *s*′, *s*″. In front of each of the beds *s*, *s*″, there is a closet, *m*, with a toilet table above it, and a movable part, *b*, underneath, which, together with the seat *c* and the movable cushion *g*, form the bed. The central bed *s*′, is contained within the seat, and brought forward as shown by *w*, *v*, by means of apparatus designed for that purpose. The central lavatory contains the washing-table L, and the water-closet C. This is the type of carriage in use on the Upper Italian railways, and it answers well for long journeys, particularly when the three passengers occupying the beds are bound for the same station. It has, however, the inconvenience that the occupier of the middle bed cannot leave the carriage without disturbing one of those at the sides.

Pullman cars, similar to those in use in America and in England, and also the sleeping-cars of the "International Sleeping-Car Company" are running on the Italian lines. These cars are attached regularly to certain trains. The railway companies receive first-class fare for each passenger travelling in them, and keep the carriages in repair, except the internal fittings.

For the benefit of passengers desiring greater comfort than the sleeping-carriages afford, there are several kinds of saloon-carriages. One of the most recent of these is shown in Plate 8, Fig. 11, in which *a* is a platform with glass sides, *b* a saloon in which three beds can be made up, *c* a bedroom, *d* a seat for attendant, *e* a lavatory.

Second-class Carriages.—In the earlier carriages the width allowed for each passenger was 18½ inches. The length of a compartment was 4½ feet, the space between the benches was 18½ inches, and the dead weight per seat, 330 lbs. Later, these dimensions were increased. Each bench held five seats, giving 20 inches to each passenger, the width between the benches was increased to 21¼ inches, the length of the compartment to 6 feet, and the dead weight per seat to 493 lbs.

Composite Carriages.—These carriages are made specially for running with comparatively empty trains, when, if a first-class carriage were attached to the trains, it might often be running empty. Composite carriages are generally first- and second-class, except upon lines of secondary importance, and those worked as light railways, upon which some of the composites have third-class compartments. The ordinary composite carriage consists of four compartments, of which the two central are first-class, and those at the ends second, constructed of the same dimensions as the compartments of the first- and second-class carriages respectively.

Third-class Carriages.—The earliest third-class carriages built were wholly devoid of comfort. There were no windows, and the only protection against rain was a leathern awning, which impeded the light and the circulation of air. By degrees, improvements were introduced, the carriages were roofed and furnished with windows, either glazed, or provided with wooden shutters; these, as constructed by the Upper Italian railways, though not, as on the French State lines, furnished with cushions, are fairly comfortable, and have plenty of light and air. Smoking used to be allowed in all third-class carriages; but of late compartments for non-smokers and for ladies travelling alone have been provided on all trains. The dead weight of these carriages is 352 lbs. per seat.

Special Intercommunication-Carriages for the principal Lines.—As the speed of the trains has increased, and the number of stopping places diminished, it has become necessary to provide greater security for passengers by enabling them to communicate with the guards in charge of the trains; and also, in the case of long journeys, to provide waterclosets, and to mitigate the discomfort of sitting for so long a time in the same position. The assaults which have from time to time been committed on solitary passengers, have given an impetus to the design of vehicles answering this description.

Electric bells and the cord system of communication have been tried, but do not afford the required protection; and it was determined to study the problem of providing intercommunication from carriage to carriage, and that in such a way as to ensure the maximum of convenience to travellers. A careful investigation of the systems in use in other countries was made, and it was soon decided that the Swiss plan of having a passage through the inside of the carriage did not furnish the required convenience to passengers upon long journeys. This system being excluded, the best appeared to be that with lateral passage first proposed by the German engineer, Heusinger von Waldegg, and the type shown in Plate 8, Fig. 12, was adopted. This carriage is entered from two end-platforms by means of doors opening on to the lateral corridor. The sides are furnished with a number of large windows, so as to give plenty of light and air. It is a second-class carriage. There are eight seats in each compartment except at one end, where one seat has to be sacrificed to allow more convenient access to the platform. At the other end is a convenient lavatory. Each compartment has a door opening upon the corridor, and when a train is made up of these carriages there is free communication from one end to the other. A special guard's and luggage-van (Plate 8, Fig. 13) is constructed for running with these trains.

A train made up of these carriages has the following advantages:

1st. Great facilities for surveillance by the company's servants, who can pass along it continually without danger, and without annoying the passengers.

2nd. Convenience for travellers.

3rd. The passengers can readily call for assistance in case of assault, or danger, the intercommunication between the compartments, and also between the carriages affording an easy means for summoning the guard.

Another type of carriage has been introduced in Italy by Mr. Luigi Teodoro Kossuth, of the Upper Italian railways, who considers that intercommunication is not required for the purpose of enabling passengers to be constantly moving about the train, but only for cases of emergency. He also thinks that passengers are liable to be annoyed by being the subjects of inquisitive observation, and that the system shown in Plate 8, Fig. 12, offers facilities for stealing, as it is impossible for the train attendants to keep a constant supervision over every carriage. He considers that access to the carriages at the ends only is not sufficient, especially when the trains are full.

In order to remedy these defects Mr. Kossuth retained the lateral corridor, but constructed it open, like a balcony. He provided doors on one side of the carriage, similar to those of an ordinary vehicle, and he also provided external doors in the corridor to correspond with those leading into the compartments. There was, however, some difficulty in obtaining access on both sides of the carriage, owing to the width allowed on the railways being limited. To get over this difficulty the inventor altered the position of the body of the carriage in respect to the underframe, cut out a piece of the floor of the corridor at each doorway, and attached to the door a piece of sheet iron to cover the opening when the door was shut. The method of communicating from carriage to carriage is still under consideration, the passage being on one side; the arrangement must be such as to allow considerable play in passing round curves. The inventor proposes placing outside the guard's van a platform, upon which an attendant can sit, and have the corridor under surveillance. At night the corridors will be lighted by powerful lamps.

Carriages for Second-class Lines, for cheap Trains, and for Tramways.—A special kind of carriage is used for cheap trains upon the principal lines. The first were two-storied carriages similar to those used upon the "Girdle Railway," in Paris; but they were very low and inconvenient. Next, Mr. Belpaire's carriages were tried upon two experimental trains, but neither did these give satisfaction. Finally, the American type has been adopted with central passage, and platforms at the end, Plate 8, Fig. 14.

Cheap trains are generally second and third class only; but upon some lines, especially during the summer months, first-class carriages are required, and in that case first- and second-class composite carriages are run.

Another type (Plate 8, Fig. 15) has been designed in conjunction with the military authorities, with the object of providing ambulances in case of war, for which purpose the seats are removed and replaced by two tiers of litters, which can either be attached to fixed benches, or slung according to the Swiss method. To facilitate the loading and the unloading of the litters the platforms are made with movable columns. The platform doors are constructed so that the opening, when in ordinary use, is 2 feet wide, but can be increased to 2 feet 8 inches when used as an ambulance.

Tramway Stock.—A great length of tramways has been constructed in Italy, and carriages of the American and Belgian types have been adopted. They are now almost all built in Italy. The Author does not propose to speak at any length of these types, which are sufficiently well known, but only of certain appliances which have been invented in Italy. One of these is the central coupling invented by Messrs. F. Grondona and Co., of Milan,[1] which is used largely in Italy, and to a considerable extent in France, Holland, and Belgium.

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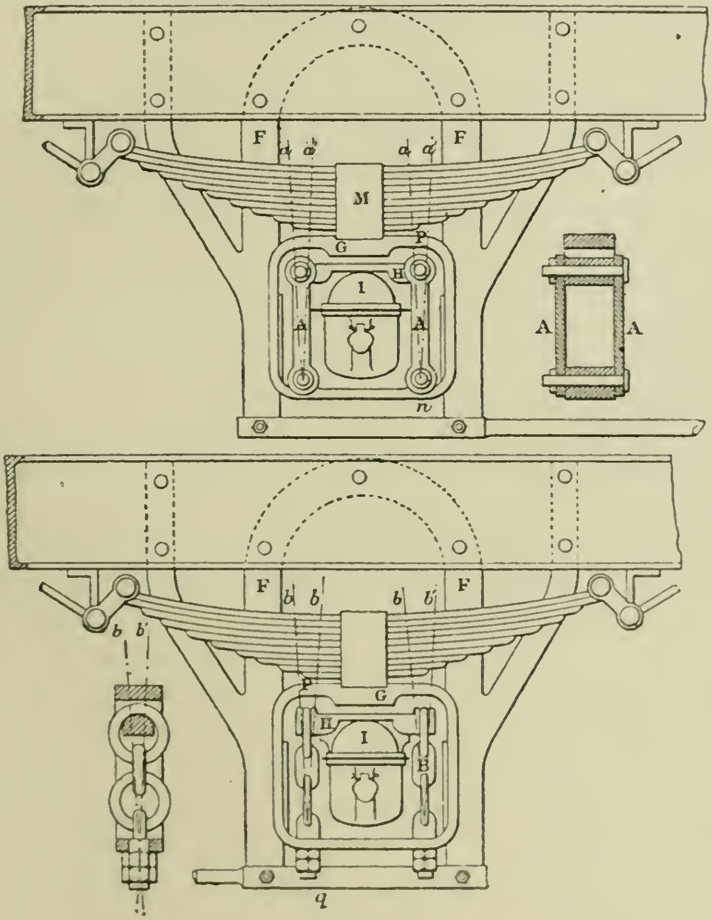
[1] This coupling is illustrated in the Minutes of Proceedings Inst. C.E., vol. lxxix., p. 194.

Another invention of the same firm is the suspended axle-box (Fig. 3), the object of which is to enable carriages to run round curves of small radius without having recourse to bogie-trucks, radial axle-boxes, or other costly appliances. For this purpose the boxes are arranged so that they do not work directly in the guard-plates, but are hung to a frame upon which the spring M bears, and which works in the guard-plate. The suspension can be effected either by chains, or tie-rods, as shown in the Figs. When chains are used they allow both a longitudinal and transverse displacement; this, however, can also be obtained by means of the rods, if a small amount of play is allowed on the pivots.

Fig. 3.



Scale $\frac{1}{30}$.

Experience shows that tie-rods are better than chains, as, particularly with light vehicles, such as are used on tramways, the latter are too sensitive, and at high speeds allow the carriage too much motion. In case of the chains or tie-bars breaking, the frame would have a slight fall on to the axle-box, which would then act in the usual way. The system might be applied to railway carriages, particularly to those with three axles, the suspended axle-boxes being applied to the central axle.

Wagons.

In the early days of railways in Italy, when the lines were short and isolated, the wagons were of the simplest construction, built of wood, without spring buffers, and very often without bearing springs. As they ran at very low speeds, no great strength was required, and for the limited traffic they answered fairly well. With the development of traffic these primitive trucks became unserviceable. At the present time Italian wagons are run through from Brindisi to Calais and Stettin at high speeds, and carrying goods of a delicate nature. The Author does not propose to deal with this part of the subject historically, but to describe the most recent practice.

Covered Wagons.—Plate 8, Fig. 16, shows the type of covered wagons now built. They can be used either for goods or cattle. When used for the former, the shutters (which turn on horizontal hinges) are closed, when for the latter they are opened.

Open Wagons.—(*a*) *Coal Trucks, with sides* 3 *feet* 3 *inches high* (Plate 8, Fig. 17).—The doors are at the sides, and are folding. The brakeman's box is placed high up, and is covered, because there are a great number of tunnels, and the men's health would be exposed to unnecessary risk had they to ride in the open.

(*b*) *Low-sided Wagons*, shown in Plate 8, Fig. 18, are used for heavy goods, metals, minerals, earth, &c.

Crane Trucks are used for loading and unloading at stations, and are sent from station to station attached to goods trains.

Wagons for Carrying Liquids.—These are principally used to carry tar and water. For some time past it has been the practice to convey wines in cisterns of tinned copper, placed upon convenient trucks. For this purpose the wagons of the "Société Generale de Transport des Liquides," which are well known in France, are employed. Mr. F. Cirio, of Turin, has designed a wagon (Plate 8, Fig. 19) intended to serve either for liquids or solids. It is built with wooden sides, lined with tinned copper. The top is divided into six compartments by a central longitudinal beam and two cross beams, upon which rest six covers hinged to the central beam, and closing air-tight upon strips of cork or gutta-percha. The covers are so arranged that when the two middle ones are shut and fastened, those at the ends cannot be opened. The middle covers are screwed down, and the screws secured by padlocks. There are two manholes, by which access to the interior can be gained without lifting the covers; by opening the manholes the taps a, b, x can be reached, by which the liquid is allowed to flow out at the bottom of the wagon.

Notes upon the Construction of Goods Wagons.

Bodies.—The framework of the bodies and the flooring is of oak. The sides are of larch or pitch-pine, which latter is very durable in the Italian climate. The ribs of the roofs are of walnut. At one time the body frames and ribs were of wrought-iron, and the sides of larch or fir, but it has been found that this construction, though very light, is not durable. The roofs are of red larch, covered with linen or hempen cloth, stretched tight, and fastened with nails round the edges, and coated several times with a composition of 8 parts of white lead and 1 part of boiled linseed oil. Over the last coat a layer of fine dry sand is spread.

Under Frames.—For the last fifteen years these have all been made of wrought-iron. Throughout Italy the ribs and transoms are of] iron, 250 millimetres (9·84 inches) high. The rest of the framework is principally made of cross-bars of] iron, 175 millimetres (6·89 inches) high, and of a St. Andrew's cross of angle-iron; for the latter, however, two longitudinal ⊔ irons are sometimes substituted. The axles and tires are of Bessemer or Siemens steel. The wheels are of wrought iron, the patterns used being those of Arbel, Brunon, Krupp, and Bochum. Mansell's wheels have been tried, but it having been found that the tires became easily displaced under the action of the brakes, their use has been limited to carriages without brakes. The maximum dimensions of the axles at the bearings are 120 millimetres (4·72 inches) in diameter, and 200 millimetres (7·87 inches) in length. In Italy the maximum loads permitted per square centimetre of horizontal section of the bearing are 27 kilograms for goods, and 18 kilograms for passenger trains (respectively 384 and 256 lbs. per square inch).

The iron used in the construction of the frames is specified to resist a tensile force of 32 kilograms per square millimetre (20⅓ tons per square inch), with an elongation of 10 per cent. in the direction of the fibres; that for the pivots must resist a tensile strain of 36 kilograms per square millimetre (22·86 tons per square inch), with an elongation of 20 per cent. The draw-bars, complete and ready for use, must resist a strain of 30,000 kilograms (29½ tons), the iron of which they are made being capable of resisting a tensile force of 39 kilograms per square millimetre (24¾ tons per square inch), with an elongation of 18 per cent. Safety chains, though generally admitted to be insufficient in case of the rupture of a draw-bar, are still used. They are required to resist a force of 18,000 kilograms (17¾ tons).

The buffers and their cases are now made of wrought iron.

The brake-blocks are all of cast-iron, except in the old carriages and wagons, in which they are of wood.

Continuous Brakes.—Experiments have been made with the principal continuous brakes with a view to deciding upon one system for adoption upon all the Italian lines, but no decision has yet been come to. It is expected that when the new organization for working the railways comes into operation, one brake will be adopted for the entire system of lines.

Statistics.—An examination of the quantity of rolling stock upon the three principal lines in 1884, shows that, dividing them into those of Italian and foreign manufacture, the numbers were :—

—	On the Upper Italian Railways.		On the Roman Railways.		On the Southern Railways.	
	Italian.	Foreign.	Italian.	Foreign.	Italian.	Foreign.
Number of vehicles . .	12,376	8,552	3,057	2,714	5,537	1,307
Percentage of foreign . .	..	40	..	47	..	23

Of the whole number of wagons, 61 per cent. are of Italian construction.

NOTES UPON THE PRINCIPAL INDUSTRIES ESTABLISHED IN ITALY FOR FURNISHING RAILWAY MATERIAL.

The number and variety of the industries employed in furnishing material for the construction and equipment of railways are so great, that it has been no easy matter for a country like Italy, in which manufactures were in a very backward condition when railways were first introduced, to found and maintain the establishments necessary for their full development. Considerable progress has been made towards this end, and at the present day a large amount of materials and plant is manufactured in the country, and the only materials which cannot be so obtained are wrought-iron and brass tubes, steel, brass, sheet-iron, and wheels and tires of engines and carriages.

It cannot, however, be said that the country is independent of foreign assistance and competition. Much has still to be done to obtain this end. The first necessity is a feeling of confidence among capitalists which will lead them to invest their money in such industries. This confidence has lately been shaken by the monetary crisis in Europe and America, which has had the effect of lowering to an extraordinary extent the cost of production in Germany and Belgium. This could not but hinder the development of the comparatively new Italian industries, which are not in a position to compete with other nations, who have on their side experience, educated workmen and foremen, and perfected methods of production. The protection afforded by Government to certain branches of industry can alone facilitate their development, and to some, such as rolling stock, even this cannot afford a lasting prosperity.

The numerous railways sanctioned since 1879, and the rapid growth of tramways, have given rise to new manufactories for furnishing the necessary plant. It is, however, obvious that the demand for rolling stock cannot be continuous, as when once a rail- or tramway is provided with the stock required for commencing operations, the administration sets up shops of its own for maintaining and renewing it. After a time the stock becomes old-fashioned, and improved vehicles are required; but the demand for these is greatly reduced, owing to the fact that radical improvements are necessarily limited to certain parts, and every effort is made to adapt the old stock to altered circumstances rather than procure new. It happened in Italy that for some years, especially after the impetus given to international communication by the opening of the St. Gothard route, all the establishments engaged in the manufacture of rolling stock were full of work, but when the more pressing requirements of the traffic were satisfied, orders ceased to come in, and the Italian factories, which, in their efforts to undersell foreigners, competed against each other, and brought down prices to an extremely low point, found themselves almost without work, and some of them were obliged to stop business. It is to be feared that a similar fate awaits other establishments, as there is no prospect of Italy being able to export this class of goods.

Exclusive of the railway workshops at Verona and Turin, which were erected for repairing only, and are not properly equipped for turning out new work, there are only three locomotive factories in Italy, namely, G. Ansaldo and Co. of Sampierdarena, established in 1855; Granili and Pietrarsa, of Naples, established in 1842; and Cerimedo and Co., established in 1879 in Milan. None of these confine themselves to engines, but only take this class of work when there is a scarcity of other kinds. The last-named has been obliged to go into liquidation.

The Author thinks, however, that were a well-planned and well-managed factory started for turning out engines for railways and tramways under the same conditions as foreign establishments, it would have a good prospect of success, especially as it need not fear competition from existing firms. Such a factory is in contemplation, and he hopes soon to see it an accomplished fact. There are, however, several builders of carriages and wagons.[1]

A few firms roll plate-iron, but they cannot nearly meet the demand for boilers and other purposes. One establishment furnishes cast-steel for axles, but its means of production are very limited; one furnishes steel for springs, but has not work enough to keep it going all the year round. There are plenty of excellent foundries. Forgings for buffers, hooks, and chains might be made in the country, but, as a matter of fact, are principally imported.

The Author concludes with the hope that, when the finances of the country are consolidated, Italy will maintain only those industries for which the demand is constant, leaving those articles for which the demand is fluctuating to be obtained from abroad, in exchange for agricultural produce, which is the principal source of the national wealth.

The Paper is accompanied by fifty-two sheets of illustrations on tracing paper, a selection from which has been reproduced in Plates 7 and 8, and the cuts in the text.

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[1] Grondona and Co., Società Veneta, Fratelli Diatto, Società Nazionale of Savigliano, Miani and Venturi, Pietrarsa and Granili.

(Paper No. 2089.)

"Experiments on the Measurement of Water over Weirs."

By Bryan Donkin, Jun., M. Inst. C.E., and Frank Salter, B.Sc., Assoc. M. Inst. C.E.

In connection with experiments on condensing steam-engines, the Authors some few years ago made a series of about sixty experiments over a tumbling-bay, $1\frac{1}{2}$ inch wide, taking accurate note in each case of the heights of water over the bay, and the quantity of water discharged. The resulting coefficients, or percentage of the actual as compared with the theoretical quantity of water, are given in the accompanying Table, Appendix I. They vary with the depth of water over the bay, and increase with a decreasing head. The object of these experiments was to determine the variations of such coefficients within the experimental limits of the apparatus used. The coefficients having been determined, the Authors compared them with those found by some other experimenters on similar bays, as shown in Appendix II. These coefficients are plotted on Plate 10.

The general formula for theoretical discharge is $Q = L\frac{2}{3}\sqrt{2\,g\,h^3}$, the same measures of time and space being used throughout, *e.g.*, all in feet and seconds. Reducing this formula to the following terms of the constants, for convenience, viz., Q = lbs. per minute; h = height over bay in inches; L = length of bay in inches; there is obtained $Q = L\;40\cdot082\,\sqrt{h^3}$.

Description of the Apparatus.

The elevation and plan of the apparatus are shown in Plate 9, Figs. 1 and 2. A is a large tank which was kept full of water, and allowed to overflow during the experiments, so that the head of water on the adjusting cock B was constant. The box C contained the $1\frac{1}{2}$ inch tumbling-bay or weir (Plate 9, Figs. 1 and 7), and was provided with baffle-boards, Nos. 1, 2, 3, 4, and 5; the boards numbered 2, 3, and 4 being plain boards, over and under which the water passed, while those numbered 1 and 5, and others near the cock B, were of perforated zinc and woven wire, the object being to diminish the velocity of approach as far as practicable, and to keep the surface of the water as quiet as possible.

The tumbling-bay was fixed at the end of box C, and was of sheet brass $\frac{1}{16}$-inch thick, with edges exactly square, and with a tie along the top to prevent the bay from altering in shape; this tie was of course much above the level of the water. The width of the opening was $1\frac{1}{2}$ inch by a Whitworth gauge, and was frequently verified. The bottom of the bay was level, and the sides were vertical. This was checked at each experiment.

MEASUREMENT OF HEAD OVER THE TUMBLING-BAY.

This was done by means of the float arrangement K, fixed in the tank C, as shown in detail, Plate 9, Figs. 3 and 4. The float was circular, 8 inches in diameter, of thin sheet-brass open at the top. It rose or fell very freely, guided by glass rods, and carried a scale, on which was indicated by a fixed pointer the exact head over the bay. The reading of this scale was facilitated by a small telescope fixed on the side of the tank, by the aid of which the movements of the float were watched continuously. The scale at K was divided to $\frac{1}{128}$ of an inch, and as a quarter of this length could be easily judged, the heads of water were measured to about $\frac{1}{500}$ part of an inch.

The zero of the scale was adjusted in the following manner:—

A point-gauge, provided with a fine screw (Fig. 6), was fixed at L in the tank C (Fig. 2). This point was set level with the bottom of the bay by a straight-edge and a sensitive spirit-level. The level in the water in tank C was now raised very gradually by pouring in water until the surface was in contact with the point gauge at L. The fixed pointer was then set exactly to the zero of the scale carried by the float. This being done, the reading of the scale as it rose and fell indicated exactly the head of water over the bay. This operation of checking the zero was repeated at the beginning and the ending of each experiment.

The head of water over the bay was further checked by a continuous-registering apparatus at M (Fig. 2). It consisted of a float carrying a pen, which marked a fine ink-line on a band of paper driven by clockwork; another fixed pen recorded the zero of the scale, so that the difference between the two lines gave the head exactly. The paper was driven generally at a speed of about 1 inch in a minute.

The water on going over the bay was either allowed to fall into the wrought-iron measuring tank D, or by means of the hinged trough E, was diverted through the channel F to the drain X. To the trough E was attached a rod, which stopped and started the stop-watch G. The measuring tank D, which was furnished with a float and index, was accurately gauged several times, and the contents were marked off in lbs. on the scale H (Fig. 5). This was done by means of a can holding 100 lbs. of water at 80° Fahrenheit, the can having been tested by the Standards Department of the Board of Trade. The sub-divisions were checked by smaller standard measures and found practically correct.

The float-index stood at zero when the tank contained enough water to cover the bottom, about $1\frac{1}{2}$ inch or 2 inches deep; from the zero of the scale to its highest reading the tank contained 935 lbs. of water. The quantity of water used for each experiment was read in lbs. by means of the scale H and float I. The tank was emptied of water by the cock J, which was watched to make sure that it did not leak.

MANNER OF MAKING THE EXPERIMENTS. (Plate 9.)

Two observers were necessary for each experiment, one person to watch the float K through the telescope, and the other to throw in and out of action the trough E, to which the stop-watch was attached. All the experiments were made personally by the Authors.

Previous to each experiment the index of the tank D was set to the zero of the scale H by pouring in or letting out a little water. Water was then allowed to run through the box C over the bay, and through the trough E, placed horizontally to the drain. This was continued until by adjusting the cock B a constant head of water was obtained, and of the required height over the bay.

At the signal to commence an experiment, the trough E was turned quickly up by the first observer, thus allowing the water to fall into the tank D, while the second observer watched the float at K, and kept the head constant. At the signal to stop, the tank D being nearly full, the first observer turned down quickly the trough E. All the water that had passed during the experiment was thus caught in the tank D. The time of the stop-watch, which was started and stopped by the motion of the trough E, as described, was also checked by a good watch. Before and after an experiment, the zero of the scale K was verified.

Experiments were made with different heads over the bay, $\frac{1}{2}$ inch being the minimum, and 3 inches the maximum. The duration of each experiment was from forty-five minutes as a maximum with the lower heads, to four and a half minutes as a minimum with the higher heads, the time being limited by the capacity of the tank D.

Effect of Errors in the Experiments.

Notwithstanding the care taken in these experiments to avoid or minimise errors, they existed to a certain extent, though not always acting in the same way. The following are believed to be the limits of errors.

1. In the measurement of the head of water over the bay [H in formula]. This could be taken to within $\frac{1}{500}$ inch. It appears in the formula to the power of $\frac{3}{2}$. An error of $\frac{1}{500}$ inch in excess in reading with $1\frac{1}{2}$ inch of water running over the bay would increase the theoretical quantity by 0·2 per cent.

2. In the length of the bay. This was $1\frac{1}{2}$ inch, and was repeatedly tested by a Whitworth gauge, which always fitted exactly; so that the value may be considered correct.

3. In measuring the quantity of water in the tank. Here the scale could easily be read to within $\frac{1}{4}$ lb. With 800 lbs. run into the tank the error would only amount to 0·03 per cent., and with 400 lbs. to 0·06 per cent. The tank itself was very carefully tested several times by different observers, and the maximum error in the index could not be as much as $\frac{1}{4}$ lb.

4. In taking the duration of the trials. This of course affected the result more in the experiments of short duration. The greatest error in this direction was probably one-half second. This in twenty minutes would amount to only 0·04 per cent., and in five minutes to 0·17 per cent.

5. In water clinging to the outer edge of the bay. Where the head was above $\frac{3}{4}$ inch, the water was always clear of the three outer edges of the bay (two vertical and one horizontal), but with the head below this, and especially at $\frac{1}{2}$ inch, the water sometimes clung to or touched the outer horizontal edge, thus slightly altering the form of the jet. Had the bay been made with knife-edges, the coefficient of discharge would probably not have been affected for any head above $\frac{3}{4}$ inch, but might have been modified at the lower heads. The Authors did not attempt to find the degree of error that this caused; very small heads should be avoided as much as possible.

In the measurement of water over bays or weirs, as the coefficients differ with the depths of water, the widths of canals of approach, the depths from the bottom of canals to the bottom edges of the weirs, the length of weirs, the thickness of weirs, and other circumstances, where accuracy is required, it would be well to follow, as nearly as possible, the conditions of actual experiments with their resulting coefficients.

The Paper is accompanied by numerous tracings, from which Plates 9 and 10 have been engraved.

APPENDIX I.

Experiments with deduced Coefficients by Donkin and Salter.

Height over Bay.	Duration of Experiment.	Actual Quantity of Water Caught in Lower Tank.	Theoretical Quantity by Formula $Q = 40 \cdot 082 \sqrt{h^3}$.	Coefficients, or Proportion of Actual to Theoretical.	
Inch.	Minutes.	lbs.	lbs.		Mean Coefficients.
$\frac{1}{2}$	45	609·70	956·54	0·6374	0·6388
$\frac{1}{2}$	30	408·31	637·70	0·6403	
$\frac{11}{16}$	30	644·50	1,028·16	0·6268	0·6254
$\frac{11}{16}$	20	427·80	685·44	0·6241	
$\frac{3}{4}$	20	492·75	781·02	0·6308	0·6307
$\frac{3}{4}$	20	492·50	781·02	0·6306	
1	20	747·24	1,202·46	0·6214	0·6226
1	20	746·24	1,202·46	0·6206	
$1\frac{1}{256}$	20	754·50	1,209·50	0·6238	
$1\frac{1}{256}$	20	756·51	1,209·50	0·6255	
1	22	826·40	1,322·71	0·6248	
1	20	753·50	1,202·46	0·6266	
1	20	755·76	1,202·46	0·6285	
1	20	739·74	1,202·46	0·6152	
1	20	743·88	1,202·46	0·6187	
1	20	747·24	1,202·46	0·6214	
$1\frac{1}{4}$	15	789·50	1,260·36	0·6264	0·6258
$1\frac{1}{4}$	15	788·00	1,260·36	0·6252	
$1\frac{1}{2}$	12	827·80	1,325·43	0·6245	0·6239
$1\frac{1}{2}$	12	828·30	1,325·43	0·6249	
$1\frac{1}{2}$	12	827·15	1,325·43	0·6241	
$1\frac{1}{2}$	12	825·90	1,325·43	0·6231	
$1\frac{1}{2}$	12	825·65	1,325·43	0·6229	
$1\frac{3}{4}$	10	863·70	1,391·86	0·6205	0·6211
$1\frac{3}{4}$	10	863·70	1,391·86	0·6205	
$1\frac{3}{4}$ less $\frac{1}{128}$	10	859·20	1,382·56	0·6214	
$1\frac{3}{4}$	10	865·70	1,391·86	0·6220	

APPENDIX I.—*continued.*

EXPERIMENTS with DEDUCED COEFFICIENTS, by DONKIN and SALTER.

Height over Bay.	Duration of Experiment.	Actual Quantity of Water Caught in Lower Tank.	Theoretical Quantity by Formula $Q = 40 \cdot 082 \sqrt{h^3}$.	Coefficients, or Proportion of Actual to Theoretical.	
Inches.	Minutes.	lbs.	lbs.		Mean Coefficients.
2	8	848·70	1,360·43	0·6238	0·6217
2	8	849·20	1,360·43	0·6242	
2 less $\frac{1}{125}$	8	837·95	1,352·47	0·6195	
2	8	845·94	1,360·43	0·6218	
$2\frac{1}{512}$	8	844·70	1,362·37	0·6200	
2	8	844·95	1,360·43	0·6211	
2	8	847·20	1,360·43	0·6227	
2	8	844·70	1,360·43	0·6209	
$2\frac{1}{4} + \frac{1}{512}$	7	878·00	1,422·20	0·6173	0·6178
$2\frac{1}{4}$	7	878·25	1,420·40	0·6183	
$2\frac{1}{2}$ less $\frac{1}{256}$	6	881·26	1,422·61	0·6195	0·6175
$2\frac{1}{2}$	6	879·25	1,425·94	0·6166	
$2\frac{1}{2}$	6	875·50	1,425·94	0·6140	
$2\frac{1}{2}$	6	884·77	1,425·94	0·6205	
$2\frac{1}{2}$ less $\frac{1}{512}$	6	876·50	1,424·23	0·6154	
$2\frac{1}{2}$	6	884·77	1,425·94	0·6205	
$2\frac{1}{2}$ less $\frac{1}{381}$	6	877·25	1,423·72	0·6162	
$2\frac{1}{2}$	6	880·50	1,425·94	0·6174	
$2\frac{1}{2}$	6	881·00	1,425·94	0·6178	
$2\frac{3}{4}$ less $\frac{1}{256}$	5	849·20	1,368·08	0·6207	0·6190
$2\frac{3}{4}$	5	842·18	1,370·91	0·6143	
$2\frac{3}{4}$	5	851·70	1,370·91	0·6213	
$2\frac{3}{4}$	5	845·94	1,370·91	0·6171	
$2\frac{3}{4}$	5	852·20	1,370·91	0·6216	
3	$4\frac{1}{2}$	863·22	1,405·84	0·6140	0·6184
3	$4\frac{1}{2}$	870·99	1,405·84	0·6195	
3	$4\frac{1}{2}$	870·24	1,405·84	0·6190	
3	$4\frac{1}{2}$	872·99	1,405·84	0·6210	

APPENDIX II.—TUMBLING-BAY EXPERIMENTS.—LIST of CURVES of COEFFICIENTS

Name of Experimenter.	Width of Bays.	Date of Experiments.	Heads over Bays. Limits of Experiments.	Coefficients varying from to	Width of Canal of Approach.	Distance from Bottom of Bay to Bottom of Canal.
	Ins. Centim.		Inches.		Feet. Ins.	Feet. Ins.
Smeaton. Plate 10, Fig. 8	3= 7·62 6=15·24 12=30·48 18=45·72	?	¼– 3 ½– 6 1 –12 1 –18	0·663–0·582 0·667–0·583 0·666–0·583 0·666–0·583	←— Not	given —→
Smeaton & Brindley. Fig. 9	6=15·24	?	1 – 6½	0·710–0·553	←— Not	given —→
Castel. Fig. 10	1·181= 3 1·968= 5 3·937=10	1835–36	2 – 8 1½– 9½ 1¼– 9½	0·629–0·6273 0·613–0·615 0·617–0·590	2 5⅛	6¾
Castel. Fig. 11	7·874=20 11·811=30 15·748=40	Do.	1¼– 7¾ 1¼– 5½ 1 – 5	0·624–0·592 0·631–0·603 0·636–0·620	Do.	Do.
Castel. Fig. 12	19·685=50 23·622=60 26·772=68	Do.	1¼– 4 1¼– 4 1¼– 4	0·642–0·632 0·651–0·644 0·660–0·655	Do.	Do.
Lesbros. Fig. 13	0·7874=2	1828–33	3¼–23½	0·652–0·637	12 1	2⅛
Comparison of Castel's & Lesbros' separate experiments. Fig. 14	7·874=20	Castel. 1835–36 Lesbros. 1828–33	1¼– 8 ¾– 8	0·624–0·594 0·625–0·585	Castel. 2 5⅛ Lesbros. 12 1	Castel. 6¾ Lesbros. 1 9¼
Ballard. Fig. 15	24	1836	1 – 8	0·660–0·801	←— Not	given —→
Blackwell. Fig. 16	36–120	1850 and 1851	1 – 6 1 – 9	0·675–0·592 0·808–0·530	30–40	Not given
Francis. Fig. 17	120	1852	7 –19	0·617–0·625	14	5
Donkin & Salter. Fig. 18	1·5	1877–78	½– 3	0·639–0·617	1 6	7
Fteley & Stearns. Fig. 19	60	1877	Foot. 0·0746– 0·8312	0·625–0·687	5	3·17
Ditto. Fig. 20	228	1879	1·63– 0·47	0·615–0·617	19	6·55
Prof. Kennedy. Fig. 21	1·75	1884	Inches. ½–2	0·700–0·610	1 5	7
Boileau. Fig. 22	34·4=87·4	1846	2¼–8½	0·632–0·669	2 11·43	1 1·384

of DISCHARGE of TUMBLING-BAYS or WEIRS by VARIOUS EXPERIMENTERS (Plate 10).

Distance Head, Measured back from Bay.	Thickness of Bay.	Number of Experiments made.	Canal and Weir same Width or not.	Duration of Experiments in Minutes.	Reference or Authority.	Remarks on Curves.
Feet. Ins.	Inch.					
← Not given →		10 12 12 15	←Not given→		Edin. Encycl. vol. xi., part 2, p. 541	Curves fairly regular. Greater heads the less the coefficients, or smaller heads the greater the coefficients.
← Not given →		9	←Not given→		Do. Neville Hyd. tab. 1875 & Min. Proc. I. C. E., vol. x.	Very irregular.
1 7¼	$\frac{1}{16}$ approx.	9 12 13	Canal much wider	about 2–10	Mémoires de l'Académie Royale des Sciences de Toulouse, vol. iv. 1834–6	Curves regular. Donkin's experiments on 1½ = 3·76 c.m., also put on this Fig. between 3 and 5 c.m. Generally the smaller the heads, the greater the coefficients.
Do.	Do.	11 9 7	Do.	Do.	Do.	Curves regular. Generally the lower the heads the higher the coefficients.
Do.	Do.	6 6 6	Canal wider	2–9	Do.	Curves fairly regular. Generally the smaller the heads the greater the coefficients.
11 6	Very thin metal	4	Canal much wider	Not given	Lesbros, Hydraulique Expérimentale, 1850	Curves pretty regular, nearly vertical. Smaller the heads the greater the coefficients.
Castel. 1 7¼ Lesbros. 11 6	Castel. $\frac{1}{16}$ Lesbros. Knife edge	Castel. 10 Lesbros. 20 ?	Do.	2–10 Not given	Do.	Curves very regular. Experiments agree fairly well, particularly from 2 in. to 4½ in. heads. The smaller the heads the greater the coefficients.
← Not given →		23	Not given	2–50	Min. Proc. Inst. C.E. vol. x.	Curve very irregular.
Not given	$\frac{1}{16}$	Not given	Canal much wider	1½–3 ½–3	Do.	Curve irregular. Generally the smaller the heads, the greater the coefficients.
6 0 ?	¼	78	Canal rather wider	3–13	Lowell Hydraulic Experiments	Curves regular. Smaller the heads, the greater the coefficients, but very slightly.
1 6	$\frac{1}{16}$	55	Canal much wider	4½–45		The smaller the heads, the greater the coefficients.
6 0	Feet. 0·0066	31	Same width	2–50	Trans. Amer. Soc. Civil Eng., Jan.–Feb. 1883, & Min. Proc. Inst. C.E. vol. lxxii., p. 336	Experiments fairly regular on curve. The smaller the heads, the greater the coefficients.
6 0	Inch. ¼	9	Do.	36–240		Agrees well with Francis' experiment, and is very regular.
1 9	$\frac{1}{16}$	28	Canal much wider	5–12	Prof. Kennedy	Curve very regular, but flatter than Donkin and Salter's.
Head measured by glass gauge	Sharp edge	7	Very little difference	?	Boileau, Traité de la Mesure des Eaux Courantes, p. 100	Curves fairly regular. This is the one exception to all the above, as greater heads give greater coefficients.

(*Paper No. 2091.*)

"Removal of Shoals by Propeller-Sluicing, on the Columbia River, Oregon, U.S."

By Harry Hawgood, Assoc. M. Inst. C.E.

In the season of low water, during the months of September, October, and November, the navigation of the Columbia river, between its mouth and confluence with the Willamette, is impeded by shoals at the Hog's Back, Walker's Island, Martin's Island, and St. Helen's.

The river at these bars had a minimum depth of 12½ feet when the bar at the mouth of the river had a depth of 22 feet of water over it. A limit was thus imposed upon the draught of vessels going in and out of Portland. To meet the reduced draught lightering had to be resorted to, causing serious delays at a time of the year during which the bulk of the wheat produced in the country was shipped.

Structures for the permanent improvement of the bars were advised by the Government Engineers in charge of the river. Pending the necessary appropriations for these works, temporary relief was sought by dredging. Raking the bottom was tried, but failed to produce any appreciable results, the current being too sluggish. On the 2nd of October, 1881, Mr. Robert A. Habersham, of the Government Engineers, recommended that the current generated by the screw of an anchored steamer should be used to erode a channel. The suggestion was advocated by the Chief of the Department. The Board of Trade, however, reported against the use of a system practically untried, the work that had been done by tug-boats on the Mississippi being on too small a scale to furnish data as to its applicability to large channels.

The nuisance of the bars continued until 1882, when the officers of the Oregon Railway and Navigation Company were induced to grant the "Walla Walla," a screw-collier of 2,124 tons register, for a trial. This vessel was 330 feet long, 40 feet beam, 16 feet draught; cylinders 40 and 70 inches in diameter, by 5 feet 6 inches length of stroke; the boiler pressure was 80 lbs. per square inch. The screw was 17 feet in diameter, having a pitch of 20 feet, and made a maximum of 54 revolutions per minute. The trial was made at Saint Helen's bar, and under the exclusive management and direction of Mr. John Gates, the Chief-Engineer of the River Division, was carried to a successful issue. A channel 1,200 feet long, 200 feet wide, with an average depth of cut of 6 feet, was sluiced-out in three days.

Fig. 1.

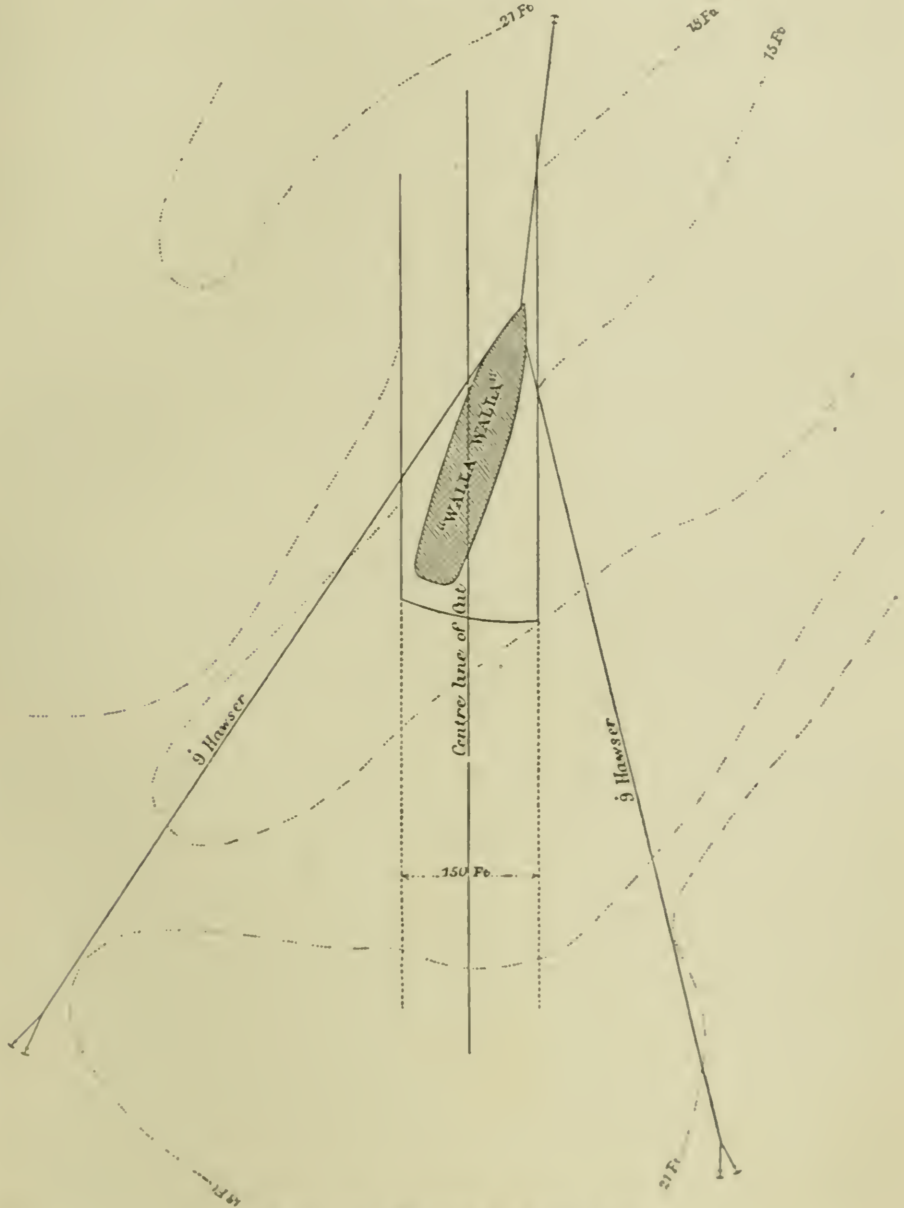
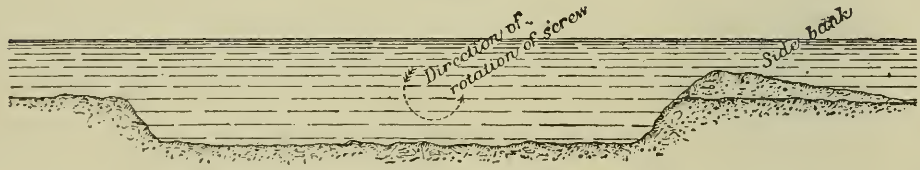


Chart of Walker's Island Bar.

The practicability of propeller-sluicing having been demonstrated, the operations were renewed in 1883 and 1884, to maintain the channel at St. Helen's. The work done each year was, respectively, a channel 1,600 feet long by 150 feet wide, with an average cut 6 feet deep, and another of the same length and breadth, with an average cut 10 feet deep. In cutting the first channel the running time of the "Walla Walla's" propeller while actually engaged in sluicing was forty-eight hours; on the second channel she was engaged for eight working days. The material moved consisted of heavy, sharp sand. It was noted that the deposit of a year was as hard to move as the original bar.

In August, 1884, Mr. Gates undertook, with the same steamer, to cut a channel through Walker's Island bar. The deposit at this point was sand and gravel, with a nucleus of solid cemented gravel. The cut was made, apparently, with no greater effort than in the looser material, stones the size of cocoa-nuts being washed out and driven down stream without difficulty. About 10,000 cubic yards of material were moved in eight working days. The method of handling the ship is shown in Fig. 1. The "Walla Walla" was backed to the bar, and moored by the bow to an anchor up-stream, and to anchors on each quarter. The vessel having been trimmed by the stern just to clear the bed of the river, the engines were started full-speed ahead. The generated current being driven away from the ship, no injury could possibly occur from the stirred-up sand working into the stern-tube or pumps; and as the cut proceeded, the "Walla Walla," pivoting at the bow, was slewed from side to side of the intended channel by her rudder, and as soon as a trench was cut across the full width of the channel, she was "fleeted" down about 25 feet into position for a fresh cut.

Fig. 2.



It was estimated that the stream immediately astern of the ship had a velocity of from 12 to 14 miles per hour, and at $\frac{1}{4}$ mile aft a speed of 3 to 5 miles, according to whether the steamer was working with or against the tide. The stream carried the bulk of the eroded material across the bar into deep water, while a smaller portion was thrown up as a bank on the side of the channel towards which the propeller turned (Fig. 2). Mr. Gates is of opinion that with a suitable propeller, and proper manipulation of the ship, this power of building a side-bank could be utilized to throw up a bank almost to water-level along the whole length of a cut—a feature that might be of great advantage in river-training.

From the inclined position of the screw-shaft the cut was deepest where furthest from the propeller, and the longitudinal profile of the cut showed a series of transverse waves; by using a rake to knock down these ridges, a further depth of about 2 feet could have been obtained at very small additional expense.

The stretching of the hawsers caused a little trouble in holding the ship in exact position, to obviate which, in future operations, Mr. Gates has devised an arrangement of rigid moorings, consisting of spars shod with flukes.

In conclusion, it may be deduced that any steamer with a stern-propeller, either screw or paddle, can be used to sluice a channel deeper than she herself requires. A large Transatlantic mail-steamer might be reckoned on to wash out a channel, in silt, to a depth of 40 feet. The "Walla Walla" opened out a channel to a depth of 7 feet below her propeller.

The establishment of the fact that an ordinary steamer can be used, with advantage, for dredging is of considerable importance, and opens out a new field in river- and harbour-improvements.

This communication is accompanied by tracings, from which Figs. 1 and 2 in the text have been prepared.

(*Paper No. 2099.*)

"Concrete-Building at Simla, India."

By Walter Smith, Assoc. M. Inst. C.E.

The present Paper is intended to give some account of the application of concrete to building purposes in the new Government offices at Simla, with especial reference to the system of building monolithic concrete walls. The points in favour of the use of concrete for these large buildings were:—(1) The possibility of having a lighter building than one of stone or brick—an important point where the site was a hillside; (2) The greater rapidity with which the construction could be carried out; (3) The plentiful supply of material for concrete, as against the almost total absence of good building-stone, or bricks at a reasonable cost.

The superstructure of the two buildings which will be referred to, namely, the "Secretariat" and the "Army Headquarters," consisted of a framework of iron with concrete walls and floors. A series of cast-iron columns with bracketed heads supported rolled-beams carrying the iron-joists for the floors, each room being contained between four groups of columns; the general arrangement is indicated in Plate 11, Figs. 1, 2, and 5. The larger building, the Secretariat, was four storeys high, while the smaller building, the Army Headquarters, was only three storeys.

The foundations were entirely of concrete. Where good soil was to be found at no great depth below the ground, they consisted of a series of piers carrying the groups of columns, with cross-walls between to support the superstructure. Where, however, the excavation was over 10 feet in depth, instead of cross-walls groined arches were thrown between the piers, thus forming a series of vaulted chambers. All specially bad places in the foundations were excavated till firm ground was reached, the space being then filled up with a solid mass of concrete to the general foundation-level. The difference of level in the foundations of the two wings of the Army Headquarters building was so great, as to necessitate a double set of vaults in the east wing, to bring it up to the level of the basement-floor of the building (Figs. 3 and 4).

The stone-ballast which formed the aggregate of the concrete was chiefly limestone broken by hand to pass through a 1-inch ring, and as most of it had to be carried from 3 to 4 miles, its cost was high, averaging Rs. 14 per 100 cubic feet. One of Blake's stone-breakers, driven by a 6-HP. engine, was also employed, but it was not successful, as it did not break the stone small enough, and the jaws wore out very fast. The stone-breaker broke up 1,200 cubic feet of stone per day.

The lime was hydraulic, burnt in kilns about 4 miles distant from the buildings, and was ground by engine-power on the works. The surkhi (pounded bricks) was obtained from brick-bats moulded near the lime-kilns, and was also ground up on the works. The lime after being ground was screened through a sieve of 250 meshes to the inch, except in wet weather, when one with 144 meshes to the inch had to be used. The surkhi was screened through a sieve of 132 meshes to the inch for fine work, and through $\frac{1}{8}$-inch and $\frac{3}{16}$-inch bar-screens for ordinary work.

Experiments gave the following results in regard to surkhi manufacture. The yield of 132 cubic feet of hard-burnt brick-bats, closely hand-packed, was 150 cubic feet of broken ballast in 2-inch cubes, and, when entirely reduced to powder, 100 cubic feet of surkhi screened through a $\frac{1}{8}$-inch bar-screen. Screening till every particle of the original bats supplied passed through an $\frac{1}{8}$-inch bar-screen occupied ten hours, and produced 100 cubic feet of surkhi, or at the rate of 10 cubic feet per hour.

The lime and surkhi were mixed together dry in the proportion of 1 part of the former to 2 parts of the latter for the concrete. It was found that 10 cubic feet of surkhi mixed dry with 5 cubic feet of lime gave 14 cubic feet of mixed material. In the manufacture of the concrete the lime was unslaked, and there was no grinding of the mortar wet; this mixture is referred to as "dry mortar." The lime, after being ground dry and screened, approached cement in its character, and was treated accordingly. The proportion finally decided on for the concrete for the walls and arched-vaulting was 45 cubic feet of dry mortar to 100 cubic feet of stone-ballast, and for ordinary foundations 42 cubic feet of dry mortar to 100 cubic feet of stone-ballast.

The usual allowance of material for 100 cubic feet of rammed concrete was 117 cubic feet of stone-ballast and 53 cubic feet of dry mortar for the walls and arches, and 117 cubic feet of stone-ballast and 49 cubic feet of dry mortar for the foundations.

The mixing of the concrete was done entirely by manual labour, machinery not being available. The stone-ballast, measured in a box containing 33⅓ cubic feet, was first spread evenly on a table 14 feet by 10 feet, laid on a slight slope, and was well washed; a layer of 14 or 15 cubic feet of dry mortar was then put on the ballast, and water sprinkled gently over the whole from ordinary watering-cans with roses, great care being taken not to wash away the mortar. The amount of water required to properly mix a "table" was from twelve to fifteen cans, or 21 to 26 gallons, according to the state of the weather. The mixing was carried out under sheds, in order that rain might not interfere with the work. The mortar was incorporated with the stone by drawing the point of a pick several times through the material as laid on the "table," and then turning it over with shovels about three times. The mixing was done almost entirely by contract at the rate of 7 annas per "table."

The number of mixing-tables in use for the Secretariat was ten, and from fifty to sixty men could be employed on them in mixing and carrying to the work, turning out from twenty to thirty tables per day, so as to allow time for the concrete to set before the framing was removed. The concrete was mixed fresh each day, and the lime was usually freshly ground, though occasionally it was allowed to accumulate, to keep the engines employed, and in this way lime of a week or two old was sometimes used. The concrete was usually carried down into the excavation in "bhangis," or hand-barrows holding about 2 cubic feet. In the Army Headquarters building, where the excavation was from 12 to 30 feet in depth, the material was sometimes slid down a shoot; a balanced platform was also constructed on the edge of the excavation, from which the material was tipped into the pit. An objection to these methods was that the mortar was apt to stick to the boards, and the stone to sink to the bottom of the mass.

The concrete was deposited in layers of 3 inches, and rammed until the mortar exuded at the top, and the blows of the rammer caused no further yielding in the mass. The rammers weighed about 11 lbs., and were made of hard wood, chiefly hill oak, bound with iron to prevent wear; the most convenient length for the handle was 3½ feet. The cost of ramming was about 3 rupees per 100 cubic feet of concrete in the work. In order to economize material and lessen the cost, hard stones and boulders were imbedded in the mass of concrete, the concrete being carefully rammed all round; the saving thus made in the Army Headquarters building in the amount of concrete below the level of the piers was 6 per cent.

The method of constructing the piers was to mark out the shape of the footings in rough stone-walling, lined on the inside with planks. As each footing was completed, and the concrete became sufficiently set, which was usually in three days, the planks were removed, and the space between the piers was filled up with earth well rammed. The next footing was then made in a similar manner, and so on until the springing of the arches was reached; earth was heaped up, well rammed, and moulded by the aid of templates into the shape required for the groining. The advantages of earth-centres were, firstly, that they were much cheaper than wooden-centres, the cost being only that of the labour in ramming and shaping the top, and the subsequent expense of removing the earth from the vaults; secondly, the ease with which the groining, which was, especially in the pentagons, of the most complicated description, could be moulded. When this had been accomplished, and the earth was consolidated, the surface was plastered with cow-dung, and over that again with lime-plaster.

The concrete was then rammed on the centres up to floor-level, each vaulted arch being completed to its full thickness before the next was commenced; the joints which had to be left between the work of different days were radial to the curves of the arches. At a later period the earth-centres were removed, when the groins were found to be clearly defined. During the whole operation, in only two places, and those at the edges, did any settlement occur, and in these cases it was due to heavy rain falling before the concrete had set.

The cost per 100 cubic feet of the concrete in the foundations was, for the Secretariat building, Rs. 49 9a. 4p., and for the Army Headquarters building Rs. 44 14a. 2p., the considerable saving in the latter being due to the use of large stones in the concrete. All work, except mixing and carriage, was done by daily labour, the rates of which at Simla are for carpenters, 5 to 12 annas; masons, 6 to 20 annas; Ladakhi coolies, 6 annas; Hindustani coolies, 4 annas.

The general design of the superstructure has already been alluded to; the concrete-work consisted of the walls between the cast-iron columns and the floors. The walls were 15 inches thick in the basement, 12 inches in the first-floor, 10½ inches in the second, and 9 inches in the third-floor; the offsets in the thickness were made on each side of the wall at the level of each floor (Fig. 2).

The material for the chimney-breasts consisted of 15 cubic feet of lime to 100 cubic feet of clinker-ballast, which was the refuse of the ground surkhi-bats after screening; this made very fine close work. The mixing was carried out in precisely the same manner as for the foundations, and the carrying was done in hand-barrows at a contract rate of 1 anna per "table" per chain-lead on the level, plus 2 annas per storey, a vertical lift of 12 feet.

The first operation in building the walls was to construct the framing. This (Fig. 5) consisted of 1½-inch planking, mostly in 10-feet lengths and 2 feet high, the planks being fastened together by battens and ½-inch coach-screws 3 inches long. The two rows of planks were kept at the proper distance apart by small wooden chocks, and were held together by ½-inch bolts 2 to 2½ feet long; eight of these bolts were used for the ordinary box 10 feet long, being placed near the top and the bottom and 3 feet apart. The joint between the adjoining lengths of boxes was ultimately effected by a slope cut on the ends of the adjacent boxes, so as to allow them to overlap. The object was to facilitate the removal of the frames, for if they were anywhere near touching, they jammed, either by the swelling of the wood, or by the disturbance caused by the ramming, unless precautions were taken. The best plan of setting up the framing, in such places as a corner of the room, is shown by the shaded portion at the left-hand side of Fig. 5, the order in which the various lengths of framing should be taken off being noted by figures. It was always a difficulty to plumb on the face of the piece of concrete between the pillars at the corner of the room, so long as framing only 2 feet high was adopted, as, owing to the position, concrete fasteners could not be used; this was overcome by planking up the piece on the outside to the top of the pillar, and securing the framing by cramps. In addition to the ordinary boxing for the walls, centres were required for the arches between rooms, and over doors and windows; at first these were made by screwing the laggings inside the boxes, thus necessitating remaking every time the walls were reduced in thickness, involving a great waste of wood. A better plan, adopted later on, was to construct a centre in the ordinary manner for the thickest of the walls, and on this to place the boxes: then, as the walls became thinner, it was only necessary to put the sides of the boxes closer together on the same centre. For the small door- and windowarches the centre was usually a solid piece of wood, with small pieces screwed on to form the chamfers in the work; on this boxes were placed as in the case of the larger arches.

The fireplaces and mantelpieces were entirely of concrete, the composition being the same as for the chimney-breasts. In the Secretariat building, the mantelpieces were omitted when carrying up the walls, and were added at a later stage; boxes giving the main shape of the mantelpiece were placed against the wall, which was nicked to afford the new material a better chance of joining the old, and the concrete was then rammed in, the details of the mouldings being afterwards added in lime-plaster. In the Army Headquarters it was thought better to construct the fireplace at the same time as the walls, and by a little extra trouble in making the boxes, the chief features of the mouldings were reproduced in concrete.

The lower part of the flue was made by ramming the concrete round a core of stones and mud; but when the narrow part of the flue, 10 inches in diameter, was reached, cores of wood were used. The core at first was merely a hollow cylinder made of planks, partly encased in sheet-iron, and having a slight taper. The core was extracted at every 2 feet, by twisting it round, and gradually working it upwards; but this treatment disturbed the concrete, and as there was only a space of 4 inches between the adjacent flues, it was a matter of some consequence. For the Army Headquarters building, an improved core was invented; it consisted of a four-sided tapering piece of wood for the inside, and four segments of a cylinder outside; the core was kept together by string while the concrete was being rammed round, and when the concrete had set, the centre-piece of wood was withdrawn by a coach-screw in the top; the sides then collapsed, and were easily taken out without disturbing the concrete.

Cores were also employed to lighten the weight of the piece of concrete in the centre of each group of four columns; two cores, 5 inches in diameter, were arranged diagonally, leaving 4 inches thickness of concrete outside; when the cores were withdrawn the hollows were covered with bits of slate, and in the next 2 feet of height the cores occupied the opposite diagonal, and so on alternately. The concrete close to the bases and heads of the columns was always solid.

The concrete was filled into the boxes in layers 3 inches thick, and rammed with light rammers until fairly solid. The walls having nothing to carry beyond their own weight, consolidation was not necessary as for other work, such as arches. In ramming the concrete for the arches in the walls, it was desirable either to finish the arch completely in one day to a thickness of at least 9 inches at the crown, or to leave the joints between the different days' work radial and not horizontal; neglect of this precaution in one case caused a failure of the arch.

The chief point to be attended to in building the walls was to set the boxes truly in line and perfectly plumb, as the slightest disturbance caused an unevenness, which had to be rectified later on by chipping off projections, and filling up depressions with plaster. In making the centres for arches, the laggings were fastened with about $\frac{1}{4}$-inch space between them, otherwise the wood when wet was apt to swell and crack the arch.

The door- and window-frames were set in place as the work proceeded, and were attached by holdfasts to the frames, and bedded in the concrete. Pipes for ventilation were put through the wall in each room, the air through the inlet-pipes being delivered into the room through small shafts in the sides of the mantelpieces.

Experience showed that considerable care was required in making the chimney-flues. In the Secretariat building, the core was not extracted until the walls had been carried up to their full height, consequently droppings of concrete, plaster, not to mention rammers, bolts, &c., accumulated on the top of the core, and, owing to the mortar, set hard and fast in the flue; and when the core came to be removed, much trouble was entailed in clearing them out. With this experience, special precautions were taken in the Army Headquarters building. As the wooden core was extracted and raised at every 2 feet, the walls inside the flue were plastered; while this was being done, a wooden disk, 9½ inches in diameter, was lowered into the flue to catch the droppings of the plaster.

From three to four days were sufficient for the concrete in the walls to set previous to the boxes being unfastened, though in very wet weather sometimes a longer time was allowed.

The first cost of boxes for 100 cubic feet of concrete in the walls was Rs. 10 12a. The cost of setting and unboxing 100 cubic feet of boxing was Rs. 2 9a. 4p.; and the cost of 100 cubic feet of concrete in the walls was Rs. 62 0a. 4p. The cost of the concrete for any building depends greatly on the quantity of work to be done and the rate of progress required, for the greater the outturn with the same set of boxes the cheaper the cost. The actual rates for the walls of these buildings were: for the Secretariat, Rs. 62 15a. 9p., and for the Army Headquarters, Rs. 64 15a. 1p. per 100 cubic feet.

The floors of the buildings were made of concrete laid on curved corrugated-iron, resting on the lower flanges of the cross-girders (Fig. 6). The concrete was, for the sake of lightness, of the same composition as that used in the chimney-breasts, namely, 15 cubic feet of lime per 100 cubic feet of hard-burnt surkhi-ballast. For ramming, a "thapi" or wooden mallet, as used by plasterers, weighing about 1 lb., was preferred to the ordinary rammer, the heavy blows of which were apt to bulge the sheets. In the case of those of 6-feet span used in the corridors, it was necessary to put temporary supports under the sheets until the concrete had set, to prevent their sagging during the ramming. The concrete was at first rammed only flush with the tops of the girders, and later on when the floors were to be finished off, about 1½ inch of fine surkhi-ballast concrete was laid on and well consolidated, the surface being finally rendered with lime-plaster. The floors thus formed were hard and good.

The estimated live-load for these floors was 100 lbs. per square foot, the calculations being made with reference to the rolled-iron joists, and a factor of safety of 4 allowed. With respect to the strength of the concrete-arches between the rolled-beams, it appears from experiments by Mr. H. Irwin, M. Inst. C.E., that one of the arches of 6-feet span and 6¼-inches rise, without the corrugated-iron underneath, was weighted with 6,007 lbs. in the centre, equal to a distributed load of 668 lbs. per square foot, before cracking.

The cost of the concrete-flooring per 100 cubic feet was Rs. 65 15a. 11p. The actual rates were: for the Secretariat, Rs. 66 1a. 4p., and for the Army Headquarters, Rs. 54 1a. 8p. per 100 cubic feet of concrete. The great saving in the latter building was due to the floors in the vaults not being plastered, and to the reduction in the stock-rates of the material.

There were three staircases in the Secretariat, two of which were spiral ascending to the top-floor, the third was carried only to the first-floor. In the Army Headquarters there were two spiral staircases, and central approach steps outside from the ground to the basement-level. These are formed of an arch resting on two arches abutting against the building. To build the spiral staircases, a set of framing was completed for one storey, of planks 1½-inch thick, held together by battens screwed on the outside. The framing being set up, the concrete was carefully filled in and rammed, and given plenty of time to set before any of the boxing was removed. Each step, which was 13 inches in the tread and 7 inches in the riser, was a beam of concrete moulded in a frame connected with the side-framing of the staircase; the steps were carried up along with the walls, so that the whole was one monolithic mass. Fig. 7 shows the boxing for the steps; each step had a bearing of 7 inches on the one below.

The proportions of the mortar were the same as for the ordinary walls of the building, but the ballast was finer, the cubes being not larger than $\frac{1}{2}$ inch to $\frac{3}{4}$ inch. The work was expensive, as the quantity of concrete was small, there being only 500 cubic feet in each storey, while the framing contained 252 cubic feet of wood.

The cost per 100 cubic feet of concrete in the staircases was Rs. 76 9a. 4p. The actual rate for the Secretariat was Rs. 79 10a. 1p., and for the Army Headquarters, Rs. 73 13a. 10p. per 100 cubic feet of concrete.

The chimney-pots (Figs. 8) were also made of concrete. These were in six pieces, each of which was formed by ramming the concrete in a copper mould. The concrete for all the sections, except Nos. 2 and 3, was composed of 1 part of lime to 3 parts of clinker-surkhi, but for those sections, owing to their tendency to break while being taken out of the moulds, Portland cement was substituted for the lime. The ramming was done with a short iron bar, and the moulds were ready to be moved in three days. The cost of the chimney-pots was about Rs. 5 8a. each. When they were erected, the various pieces were joined together *in sitû* with neat cement, and a coating of very thin cement was laid over the whole with a brush to give a uniform appearance.

The deposition of concrete in the foundations of the Secretariat building was commenced in March 1882, and the concrete in the superstructure was practically completed in December 1883. The quantity of concrete used was: for the foundations, 108,816; walls, 122,149; floors, 32,387; total, 263,352 cubic feet.

In the Army Headquarters the dates and quantities were: commencement of laying concrete in the foundations, March 1883; completion, including the vaults, May 1884; quantity of concrete in the foundations, 175,865 cubic feet. Commencement of concrete in the superstructure, July 1883; completion, July 1884; quantity of concrete in the walls, 72,862; floors, 16,954; together, 89,816 cubic feet. The total quantity of concrete in the building was 265,681 cubic feet.

The work was considerably interfered with, in the winter by frost and snow, and in the summer by the scarcity of water. To neutralize the effect of frost, salt-water was used for mixing the concrete, apparently with success; snow, however, stopped the work entirely for days together. To keep up a supply of water in the hot weather, a length of ½ mile of 2-inch piping was laid to a stream in a neighbouring ravine, and the supply was further augmented by carriage from springs by men and mules. To keep the large number of labourers together, a considerable amount of hutting had to be provided, and arrangements made for food. During the earlier stages of the work two companies of "Pioneers" were employed, mostly on earthwork and concrete, mixing and carrying being paid by taskwork.

In conclusion, the Author, who acted as Assistant Engineer in charge of the works, has to acknowledge his indebtedness to Mr. J. M. Campion, M. Inst. C.E., the Executive Engineer in charge of the construction, for much of the foregoing information; and also to Mr. H. Irwin, M. Inst. C.E., the designer of the buildings and Superintendent of the works.

To make the subject of the application of concrete to building-purposes in Simla complete, it may be mentioned that it was used for filling in the walls in half-timbered structures, as in the new Post-office and the Ripon Hospital. Planking was screwed on the two sides of the posts, sufficient opening being left on one side for inserting the concrete and ramming it. To avoid any crack between the concrete and the wood from the shrinking of the latter, a narrow groove was cut in the side of the posts into which the concrete went, and as a further precaution, old nails were driven into the wood and imbedded in the concrete to form a bond.

The usual thickness of the concrete-panels was 4½ inches, including the plaster; on the inside the plaster was flush with the timber-work; but on the outside, the posts projected, a chamfer being run along the edges. The materials and the proportions of the concrete were similar to those in the buildings previously described, though the stone-ballast was necessarily finer.

The ramming was done with short thick sticks. A somewhat novel kind of floor in the Ripon Hospital deserves notice. It consisted of wooden joists, 7 inches square, laid as shown in Fig. 9, carrying curved tiles of concrete, abutting on the sloping sides of the joists. Over the whole floor was then spread a layer of concrete composed of hard burnt surkhi-ballast and lime, in the proportion of 15 of lime to 100 of ballast; this was well consolidated with wooden mallets, and then finished off with a coat of ordinary lime-plaster or Portland cement. The tiles were made in wooden moulds by beating with wooden mallets, the proportions used being 1 part of dry mortar to 2 parts of fine stone-ballast. The cost of the tiles, including moulds, taken on an out-turn of 4,650, was 6 a. 3·4 p. each; the rate for the ballast being Rs. 9 per 100 cubic feet, and for the mortar, 8½ a. per cubic foot.

The Paper is accompanied by two sheets of tracings from which Plate 11 has been compiled.

(*Paper No. 2111.*)

"On an Improved Method of Lighting Vessels under Way at Night."

By Bradford Leslie, M. Inst. C.E.

The existing system of lighting ships at sea has been in force for many years, and was adopted when steamers were the exception, and sailing ships formed by far the larger number of vessels navigating the high seas, and it may be conceded it was fairly well adapted to the times. The coloured side-light indicates the tack on which a vessel is sailing, and knowing the direction of the wind, it is generally possible to form a tolerably correct idea of her course, so that all the steamer or vessel running free has to do is to give way to the vessel close-hauled. In the case of sailing-vessels there is no uncertainty as to which vessel ought to hold her course or which ought to give way, a point by no means so clear in the case of steamers meeting.

The coloured signal-lamps in use on railways are required to show a light over a small arc in one direction only. Having their rays concentrated by concave reflectors, they are visible for several miles, but the side-lights of ships are diffused over an arc of 110° with a corresponding loss of power, and as the coloured glasses absorb about 70 per cent. of the light, the result is that ships' side-lights cannot be depended upon for a greater distance than 1 mile or 1½ mile, according to the power of the lamp and the state of the weather. The average combined speed of sailing-vessels approaching each other is not more than half that of steamships, so that the limited range of the coloured side-lights was of minor importance so long as sailing-vessels only were concerned.

Steam-vessels, however, approaching each other at a combined speed of 20 to 25 miles an hour, must meet or pass each other in a period of five to three minutes, according to circumstances, after first sighting each other's side-lights; five minutes is too little for safety, and three minutes is practically useless for any considerable alteration in the course or speed of a long steamer travelling at 10 or 12 knots per hour. True, the mast-head lights of steamers are visible for a much longer period, but they afford no indication of the vessel's course.

It must also be borne in mind that the loom of a ship under canvas can be seen for some distance on a night in which a steamer without sail, and with small provision of spars and rigging, would be invisible. All these circumstances lessened the risk of collision at night in the times when steamers were few and far between.

Perhaps, however, the most serious disadvantage of the existing system of ships' lights, particularly in the case of steam-vessels, which shape their course from port to port without reference to the wind, is that the coloured side-lights afford only the vaguest indication of their course, and none whatever of their manœuvres, except in the case of a vessel altering her course from one side of the line of vision to the other, which is indicated by a sudden change in the colour of the lights. A vessel showing her green light may be passing from port to starboard, or may be approaching almost end on, or may be altering her course from one at right-angles to the line of vision to one almost parallel to it, or *vice versâ*; but the steady green light affords not the slightest indication of these divergences or changes of direction. The only meaning of a green light is that an approaching vessel, within a distance of a mile or two, has her starboard-side turned towards the observer; but at what angle or how that angle may be changing, there is nothing to show. The same uncertainty exists as to the movements of a vessel showing her red light.

These defects are serious objections to the existing system of ships' lights; since the vast extension of steam-navigation they have been the cause of an enormous sacrifice of life and property. It is only the great vigilance, skill, nerve, and presence of mind of the experienced navigating-officers, that enables them under such disadvantages to avoid more frequent disaster.

The course of every vessel under way should be as clearly apparent by night as by day; and considering the rapidly increasing traffic on the high seas, and the constantly higher rates of speed at which steam-vessels travel, some improved system of lighting ships at night should be introduced, by which it will be possible to ascertain at once and unmistakably, not only the course of any vessel under way, but also any changes in such course. It must be borne in mind that these collisions by misapprehension of movements seldom or never occur by daylight, the reason being that the vessels are in sight of one another in plenty of time to adopt the measures, if any, that may be necessary to avert collision.

Red lights and green lights, being rarely visible at distances exceeding 2 miles, cannot be relied on for indicating the position or manœuvres of vessels approaching each other at a combined speed of 20 or 25 miles an hour; but there is no objection to the old system of coloured side-lights being preserved, so long as it does not interfere with the adoption of some improved method of making clear by night the position, course, and manœuvres of vessels under way.

White lights are plainly visible at a distance of 8 or 9 miles—say for a period of twenty to twenty-five minutes in the case of vessels approaching at the combined speed of 20 or 25 miles an hour—and are therefore the best for indicating the course and manœuvres of vessels under way to other vessels within such a radius. It is suggested that every vessel under way should carry one white light at the mast-head or on the fore-stay, showing a light directly ahead and over an arc of 110° on each side, and a second white light, visible over the same angle, at or near the stem, so located as to form an angle of 45° with the mast-head light and the horizon.

Steamers would carry three lights—one light at the mast-head, one on the fore-stay, and one at the stem, forming an angle of 45° with the horizon, to distinguish them from sailing-vessels. The use of two white lights, one light over the other, to indicate a steam-boat towing a ship, would have to be discontinued, and some other symbol substituted.

The system of lighting above suggested is simple and incapable of giving rise to misapprehension; it would indicate exactly the position, course, and manœuvres of every vessel under way to every other vessel on a forward arc of 220°, or from two points abaft the beam on each side, and within a radius of 8 or 9 miles. A ship's lights appearing at an angle of 45° with the vertical line would indicate that the vessel sighted was steering a course at right-angles to the line of vision; any lesser angle with the vertical as, for instance, 39°, indicating a course forming a lesser angle (54°) with the line of vision, until the appearance of the lights in a vertical line, one over the other, would show that the vessel was approaching stem on. If, as suggested, the lights are adjusted to form an angle of 45° with the horizon or with the vertical line, then the cotangent of the apparent angle, which the lights of an approaching vessel make with the vertical line, is the sine of the actual angle which the course of the vessel makes with the line of vision, so that the exact course of an approaching vessel may at any time be readily ascertained. It is a practical advantage of the proposed system of lighting ships, that in the more critical cases when a vessel is approaching nearly stem on, the apparent or visible angle, formed by the lights of the approaching vessel with the vertical line, coincides almost exactly with the angle which the course of the vessel makes with the line of vision, and even up to an angle of 20° there is a difference of 1° only between the apparent angle of the lights with the vertical line, and the actual angle made by the course of the approaching vessel with the line of vision. This graphic representation of the course of an approaching vessel, by the angle which her lights form with the vertical line, would be invaluable to pilots and practical navigators when there is not much time for plotting angles, or making calculations.

In the case of sailing-vessels heeling over under pressure of wind, some allowance would have to be made for the inclination from the vertical plane. A correction might be arranged by making the lower or stem light travel laterally on an arc, so that it would always be in the vertical plane under the mast-head light; but in practice it will probably be found that experienced navigators, knowing the direction and force of the wind, will have no difficulty in allowing for this source of error.

Night telescopes might be furnished with an apparatus for measuring the angle made by the lights of an approaching vessel with the vertical line, so that the angle of the ship's course with the line of vision would be ascertained, by direct inspection of the position of a marker on a graduated arc. The effect of the adoption of the proposed system of ships' lights would be, that the position, course, and manœuvres of every approaching vessel would be known at least fifteen minutes earlier than under the existing defective system, the coloured side-lights affording an uncertain indication of her being within a few minutes of possible collision.

(*Paper No. 2126.*)

"The Oil-Wells of Baku."[1]

By F. Vasilieff, Mining Engineer.

(Translated and Abstracted by William Anderson, M. Inst. C.E.)

This is an important Paper of sixty-one pages, accompanied by maps and four large sheets of drawings, illustrative of the various modes of boring and of the tools used.

The peninsula of Absheron, jutting out into the Caspian Sea, an arid waterless desert, has from the most remote antiquity attracted the attention of surrounding nations on account of the treasures it contained. The inflammable gases issuing from the ground served to feed perpetual flames, and in the eyes of the fire-worshippers, rendered the whole locality sacred. Ordinary wells were the earliest means employed for obtaining the naphtha, and a Persian inscription has been found which fixes the date of one of these at 1594.

After the conquest of the country by the Russians, the oil-wells became crown property, and between the years 1820 and 1873 the springs were sometimes in the hands of the government, and at others in those of farmers, and the revenue of fifty-three years did not exceed £10,000. The number of wells in 1872 was 415. The manufacture of kerosene commenced in 1858, after which the industry began to develop slowly. The quantity of naphtha raised in 1863 amounted to 5,400 tons; in 1872 it rose to 23,800 tons; the value of the land also increased, till in 1872, when the allotments were first let by auction, an acre sold at from £900 to £1,100. From this period also dates a greatly increased activity. Wells were abandoned for bore-holes, which now number five hundred, and hand-power was replaced by steam. In that year, also, the famous Vermishoff fountain burst out, flooding a large extent of country, giving, for the first time, an idea of the immense riches contained in the peninsula. The yield has now reached 950,000 tons per annum, notwithstanding the ignorance and extravagance which characterized the early efforts of the miners. The geology of the oil-region has been studied and described by Messrs. Abich Koshkool and Batzevitch, while Professor Romanofsky has defined the principles upon which the sinking of bore-holes should depend.

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[1] The original appears in the Gorny Jurnal (Russian Mining Journal) for September 1885.

The centre of the oil industry, in the Absheron peninsula, does not exceed 4½ square miles in area, and forms, indeed, the centre of the whole oil-region of the Caucasus. The naphtha-bearing strata belong to the lower miocene series of the tertiary epoch. The upper beds, consisting of fossiliferous limestone, have disappeared in the oil-region, the rocks of which are covered by deposits of alluvial origin. The naphtha-bearing beds extend in a N.E. to S.W. direction, and the dip, as well as it can be made out at present, ranges between 20° and 40°. The beds are composed of sand, calcareous clays, marls, and in places, compact sandstone, often of great thickness, frequently penetrated by bands of pyrites. As in Pennsylvania, the naphtha appears associated with salt water furnished by the water-bearing sand-beds, which are a source of considerable trouble in driving the bore-holes. The oil-bearing plateau is about 140 feet above the level of the Caspian, consequently all the bore-holes of greater depth are below the sea-level. The absence, however, of sulphates in the waters coming from the sand-beds, appears to indicate that the salt-water so often met with, does not penetrate from the sea.

The calcareous clays are easily bored through, but they are very apt to break into the holes, and have to be supported by means of bore-pipes. The naphtha-bearing sands are in a semi-fluid condition, and when brought to the surface, give off carburetted hydrogen gas. These sands are found to the extent of from 30 per cent. to 40 per cent. in the oil-springs and fountains, giving much trouble by silting up the bores. A characteristic of these groups of naphtha-bearing rocks is the total absence of organic remains down to the maximum depth yet reached of 840 feet, while the upper beds of the series abound in shells of the Aral-Caspian order.

Practice has abundantly justified the principles advocated by Professor Romanofsky for determining the best position of bores. They should not be near the outcrop of the oil-bearing strata, nor at anticlinal bends; but should strike the beds at not less than 400 feet below the outcrop. The pent-up gases play a very important part in the formation of springs or fountains, which is demonstrated by the fact that the level of the oil in the wells depends upon the direction and force of the wind.

The number of oil-bearing strata is unknown; three have, up to the present, been defined, and it is found that the deeper the stratum the lower is the specific gravity of the oil, and the richer it, consequently, is in kerosene.

Owing to the nature of the rocks, it appears that the sinking of bore-holes of 18 inches diameter is best effected by means of boring-rods, and Fabian's freely falling tool. At first sight it would seem that the American rope-system should be the best; but although introduced by the brothers Nobel, it has not met with wide adoption, the reason being that, in the United States the bore-holes are smaller—only 5 inches to 8 inches diameter. There also the beds are more horizontal, the hardness of the rock more uniform, and the oil flows freely or can be pumped up. The condition of the oil in the Caucasus obliges it to be got out of the bores by means of scoops, hence the bore-holes have to be larger, usually 16-inch diameter, and having to be maintained at that diameter throughout, must be lined with bore-pipes, necessitating the use of widening-out tools. The softer strata render it more difficult to keep the bore-holes straight, yet this quality is, under the circumstances, very important. The oil-bearing sands can frequently be got out by means of a current of water set in action simultaneously with the sinking of the bore-pipes. The owners of allotments are free to sink their bore-holes where they like. A glance at the map reveals that they are mostly sunk along the boundaries of the plots, and not at the points which the lie of the strata would indicate to be the most advantageous. The reason for this is, that each proprietor considers that by sinking a bore near his neighbour's plot, if he succeed, he will get his own oil and a good deal of his neighbours also; his neighbour is actuated by the same motives, hence the allotments have the appearance of fortified places, being surrounded by works, and unoccupied over the greater portion of their inner areas. Naturally, if a bore be exceptionally successful, a large number of additional ones are at once driven, and the yield of each is in consequence reduced.

The Author proceeds to describe, in great detail, the structures which are erected for sinking the bores, the machinery used for working the boring-tools, and the bore-pipes employed, the whole being profusely illustrated by drawings to scale.

The borings usually commence with a well 6 feet to 7 feet square and about the same depth, which serves to steady the bore-pipe, and the latter is sunk in the usual manner by scooping out the ground inside it. Sometimes the pipes are not carried through the water-bearing sands, and in these cases disasters are frequent from the sand constantly escaping from around the pipe, forming cavities which, at times, even undermine the foundations of the turrets.

These effects are very much aggravated when the pressure of the subterranean gases, amounting at times to as much as 300 lbs. to the square inch, causes the oil to rise in fountains and produce a more energetic scour. When the method of closing the bores of the fountains with valves, so as to regulate the flow, was adopted, it became necessary to devise measures for preventing the oil from being forced through between the pipes and the surrounding ground. This has been successfully accomplished by first sinking octagonal wells about 6 feet in diameter some 40 feet down to the hard ground, then placing the bore-pipe in position and filling in between the well and the pipes with masonry in cement. In consequence of Portland cement not sticking to oily substances, and the whole place being saturated with oil, it is impossible to prevent some leakage of naphtha and gases through the masonry, but the amount is not of importance. The same object has been attained still more effectually by tamping the space between the bore-pipe and well with puddled clay, rammed in thin layers after the joint between the bore-pipe and the hard ground had been well calked with rope-packing. The bore-pipes vary from 18 inches to 14 inches in diameter, and are made of sheet iron from $\frac{3}{16}$ inch to $\frac{1}{4}$ inch thick; the diameter decreases when great depths are reached; as for example, a case is mentioned of an 18-inch bore ending in 14 inches at a depth of 451 feet, and another at 13 inches at 525 feet. A bore of the above dimensions would yield 222 tons of oil a day.

The Author devotes considerable space to a minute account of the bore-pipes in use and the various methods of driving them; he also describes the process of drawing and renewing pipes which have been injured or got too far out of perpendicular. In a particular case, a bore was sunk 490 feet through quicksands, and in this instance a screw press—which is figured and described—was used for the first time with great advantage, because the work of getting out the sand was able to proceed while the screwing down of the bore-pipes went on.

The cost of the bore-pipes is a very serious item of the expenditure; he estimates that in a bore 600 feet deep composed of 16-inch, 14-inch and 12-inch pipes, these cost £510, while the labour of sinking amounted to £1,000. The bore-pipes are not unfrequently crushed in from excessive external pressure. A minute description, illustrated by drawings, is given of the tools used for repairing injuries of this kind.

The depth to which bore-holes are sunk depends upon the yield of the strata and the value of the oil. For the most part the explorers do not like to stop till they find the naphtha rising to the surface. An opinion is formed of the probable yield by the quantity of gas given off, and the level to which the oil rises. At first the quantity of gas is small and the level of the oil low, but ultimately the level rises, and a sound as of boiling comes up the bore-pipe. As each oil-bearing bed is reached these phenomena are intensified, and a trial of the yield is then made. A sudden fall in the level of the oil is considered a bad sign, though cases have occurred in which, after such indication, a fountain has ultimately been obtained. It sometimes happens that a bore, which promises to become an abundant fountain, yields 50 per cent. of water, and this phenomenon has become more and more frequent in the oil-bearing region, giving rise to numerous complaints. The cause seems to be the exhaustion of the naphtha from the oil-bearing strata and the infiltration of water into them. Even in wells which have been in action for a long time, and which have, till lately, never even given indications of water, this substance is beginning to form a not insignificant part of the yield.

The only precaution which can, as yet, be taken, is the endeavour to make the wells watertight; the knowledge of the geological formation of the district is still so slender that general measures to intercept the water cannot be adopted, and watertight bore-pipes do not prevent the water finding its way down to the bottom between the pipe and the rocks, and rising up inside with the oil.

Boring goes on night and day. A gang, consisting of a foreman, two assistants and ten men, costs £30 per month. The cost is given in detail of sinking a 16-inch bore 600 feet, amounting to £1,148 12*s.* brought up to £1,300 by office and other charges not directly connected with the actual work. The rates of boring by contract are also given. The average rate of sinking is stated to be 140 feet per month. Ordinary labour is obtained from the villages in the locality, but the foremen and skilled artizans all come from a distance, but are, nevertheless, to be had in abundance. The rates of wages are as follows: engine-drivers with a house, £8 to £10 per month; fitters, 5*s.* per day; turners and smiths, 6*s.* per day; carpenters, 4*s.* per day; labourers, from £1 16*s.* to £2 per month; Persian labourers, £1 to £1 4*s.* per month. The Mahommedan workmen are preferred because of their sobriety and the absence of holy days.

A naphtha fountain differs very much from one of water. The oil on leaving the pipe is broken up into many jets which scatter in all directions; the larger part, on account of the liberation of the occluded gases, is shattered into the finest spray. Together with the oil there is ejected an immense quantity of sand, stones, lumps of clay, some of the pieces being very large. This condition of things is explained by the high pressure of the gases which has been measured in closed bore-pipes and found to range between 50 lbs. and 300 lbs. per square inch. In the year 1883 two fountains played simultaneously to a height of between 250 and 350 feet. When a fountain breaks out the boarding of the boring-turret is soon torn off, stones are thrown up to a great height, and it is dangerous to approach the bore, especially from the circumstance that the naphtha spray has an inebriating effect on the workmen. A cloud of naphtha hovers over the fountain, and is carried to great distances by the winds, covering everything it passes over with a light film of oil; the sand thrown up forms a hillock round the well often rising to 28 feet in height. The bursting forth of a fountain is accompanied by loud noises and a trembling of the earth. Millions of tons of oil have been lost from an inability to direct it into reservoirs, which are frequently not even prepared before the need for them arises. Some fountains are intermittent, and play from one to two or three hours at a time and then cease for a day or so. These are the most convenient, as they give plenty of time to arrange for collecting the oil. In some cases the action has to be started by withdrawing a few scoops of oil from the bore and thus disturbing the subterranean equilibrium.

Continuous fountains, sooner or later, become intermittent, and then, like the latter, settle down into ordinary wells from which the oil must be raised by the usual methods. The jet sent out of a bore-pipe appears urged forward by a rapid succession of pulsations; but periods of quiet may be noted, during which the fountain seems to gather up its strength for an extra energetic effort. The height of the jet varies with the intensity of the pulsations. A continuous fountain may yield over 3,300 tons of oil, and require the labour of one hundred men to collect and store it in reservoirs. The daily yield would be worth £100, the cost of labour being £15 to £20.

The idea of regulating the flow of wells occurred, naturally, at an early date in the history of the district. The first attempts consisted simply in plugging the bore-pipes by means of wooden plugs covered with felt; the operation was a dangerous one, volunteers had to be tempted to do the work by large prospects of reward. Subsequently sluice-valves, which are described in detail and illustrated, were fitted to the bore-pipes; but considerable difficulties are encountered in the use of these, on account of the eroding action of the streams of sand and oil, and the tendency of the naphtha and gases to make their way to the surface outside the bore-pipes and the lining of the well.

The bores are apt to silt up gradually, the sand settling in so compact a form that it has to be removed by the aid of the boring-tool, and even, at times, it has to be loosened with the chisel.

As a rule, the level of the oil rises in the bore as the latter is driven deeper, and will frequently stand some 10 feet to 15 feet below the surface. If the oil be taken out too fast, the level sinks even to the bottom of the bore. An instance is given of a well which for ten years yielded 140 tons per day without any sensible change in the level of the oil, which was 51 feet below the ground line; but lately the supply has failed altogether. Some wells must be allowed to rest for several days, and, as a rule, it is best to work the bore in the daytime only. The mean produce of the one hundred wells now in active work is given at 32 tons per well per day. Assuming that the season extends from March to November, the Author calculates the total annual produce of oil at about 1,000,000 tons, and the average cost of production he estimates to be £2 12*s.* 6*d.* per ton.

One of the most serious sources of difficulty and expense is the scarcity of soft water, not only for feeding the boilers, but even for dietetic purposes; the expenditure for water rises, even under favourable circumstances, to 4½ per cent. on the cost of the oil. Large tanks or reservoirs are constructed in which the rain- and snowfall of the winter is collected and stored for use during the rainless summer, in which season only the works are in full activity. A film of oil forms on the surface of the water in the tanks, and serves to prevent excessive evaporation. Shallow wells, reaching to the uppermost water-bearing sands, are also used; but the water from them is heavily charged with dissolved mineral matter.

The Paper concludes with a Table of strata penetrated by one of the borings.

APPENDIX.

Proceedings of the Commission appointed by the Imperial Russian Technical Society to inquire into the Project of a Naphtha Pipe-Line from the Caspian to the Black Sea, and into the Measures to be adopted for the Development of the Naphtha-Industry in Russia. St. Petersburg, 1885.[1]

The preface to the volume states that a Paper by Mr. Ilimoff, "On the Condition of the Naphtha-Industry in Baku, and on the Pipe-line," was the immediate cause of the appointment of a Commission, composed of twenty-one members of the Technical Society, and four persons interested in the question, but not connected with that body. The commission was presided over by Mr. Audreef, and held numerous sittings, during which evidence was heard and various reports were examined.

The proceedings commence with a report of Mr. Ilimoff's Paper, which was read on the 22nd December, 1884 (O.S.). The Author points out the great difference which exists between the Russian and American naphtha, the former yielding only one-third of the weight of kerosene which the latter is capable of producing, and yet that the Baku manufacturers blindly follow the American method of treatment, and reject three-quarters of their raw material either absolutely or for the inferior purpose of fuel. He remarks that this savage waste of riches has frequently been the subject of comment by competent authorities, who have shown how, by a rational and scientific treatment, the Caucasian naphtha may be made to produce valuable products other than kerosene. Baku, however, he considers, is not the locality in which a wide development of manufacture can take place. The climate is not favourable, there is no fresh water, the place is isolated, far from all manufacturing centres, and from the markets in which ready sales can be effected; he advocates, therefore, the laying of a pipe to Batoum, on the Black Sea, on the grounds that the oil will be transported cheaper than by the Trans-Caucasian Railway, that the oil itself will serve as fuel for the pumping-engines, that cheap coal is to be found on the shores of the Black Sea, that the chemicals necessary for the complete utilization of the oil will there be more readily procurable, that the waste products will find a more ready sale, that the ports are open throughout the year, and finally, that any naphtha-producing regions that the pipe may traverse or approach will at once be made capable of being worked. The views expressed by the Author appear to have found favour with the Ministry of Crown Lands, which is greatly interested in the development of the commercial prosperity of the Caucasus.

The interesting question of the probable duration of the supply from the Baku district is next discussed. The whole oil-bearing region does not extend to more than 6½ square miles. Mr. Konshin estimates that at the annual output of 1½ million tons, the region would be exhausted in thirty years, while Mr.

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[1] The original report will be found in the Library Inst. C.E.

Sokolovsky, Mining Engineer, sets the duration of the supply at only seventeen years.

The Paper next discusses the question of export-duties, and comes to the conclusion that, in any case, the raw oil will be unable to compete with the finished American products in the markets of Western Europe. The flashing-point of the illuminating oils, which does not appear to have been authoritatively settled by the Russian government, is dwelt upon, its influence on the yield is pointed out, as well as the importance to the safety of the public, and especially of the peasantry, among whom the use of naphtha-lamps is very rapidly increasing.

The cost of transport from Baku to Batoum by means of a pipe, the Author estimates as being between 10*s.* and 12*s.* 6*d.* per ton; while by rail, in special tank-wagons, the cost will be between 23*s.* 2*d.* and 31*s.* 6*d.* per ton; but he remarks, that if the tonnage were to exceed 800,000 tons per annum, the line would have to be enlarged. The Paper concludes with valuable Tables of statistics, weights and measures. The Author treats the oil-producers of Baku with considerable severity, and a good deal of the discussion which followed was devoted to a defence of the existing system, and ended in the appointment of the commission to which reference has been made at the commencement of this abstract.

The commission held eight sittings, the last on May 3, 1885 (O.S.). The discussions, for the most part, were very lengthy, and in addition, individual members reduced their views to the form of reports. The results of its labours are collected in a general report, which, after sketching the progress of the naphtha-industry since 1832, goes on to state that in 1883 the Minister of Crown Properties visited the district and became strongly impressed with the necessity for legislation, to arrest the wasteful manner of working, to promote the manufacture of naphtha-products, to cheapen the means of communication, and, without interfering with private enterprise, to enable Government to draw some revenue from the immense riches within its territories. To attain these objects, it was proposed to regulate the getting of the oil, to promote the introduction of improved modes of manufacture, to impose a heavy duty on the export of raw or half-manufactured oil, to determine a legal flashing-temperature, and to lay a pipe-line from Baku to Batoum.

The distance from Baku to Batoum is about 556 miles. Various proposals respecting the pipe-line have been made. Some consider that it should be laid along the Trans-Caucasian Railway, and be worked in conjunction with it, others prefer an independent route. Some consider that it should be a Government enterprise, others that it would be best in private hands, with guarantees for impartial working. The diameter of pipe suggested varies from 6 inches to 8 inches, the number of pumping-stations from thirty-five to fifty, the maximum pressure 1800 per square inch, and the maximum quantity pumped per annum 800,000 tons. The estimates of the cost of the line ranged between £1,570,000 and £2,020,000, the oil consumed as fuel in pumping is estimated as high as 30 per cent. of the quantity pumped, and the estimated cost of pumping the entire distance ranges from 5*s.* 6*d.* to 15*s.* 6*d.* per ton, the latter figure being calculated on the quantity received at Batoum. These estimates assume from 5 per cent. to 10 per cent. interest on the outlay, and that the capital will be recouped in from fifteen to twenty years. The commission is of opinion that with only about 300,000 tons pumped per annum, the pipe may be made a paying undertaking, and that no other interests, not even those of the railway, will be affected injuriously. They are of opinion that the line should be a private enterprise, because in that case it will be undertaken only when an undoubted necessity has arisen, and when the supply of oil will be assured, and they state that, in their judgment, there is ample room for factories on the Black Sea as well as at Baku, the former will be the centre of the export trade, the latter of the internal and Asiatic commerce.

The minimum rate at which the railway can carry in special trucks, is set down at between 13*s.* 3*d.* and 16*s.* per ton for the whole distance; but these rates will not cover a fair interest on capital, and can only be entertained as surplus revenue.

With regard to the objection that in a pipe-line different qualities of raw oil would get mixed together, it was stated that in the United States no inconvenience has been found to arise from this cause.

The Report of the Commission is comprised in two hundred closely printed pages. Valuable Tables respecting the railway-rates are given, and all the calculations of cost, resistance of pipes, &c., are stated in detail. The opinions expressed by a great number of competent persons during the discussions are most valuable, indeed the whole subject is laid bare in an exhaustive manner and from many points of view.

W. A.

(*Paper No. 2132.*)

"The Salt-Industry of Stassfurt."[1]

By Hilary Bauerman, Assoc. M. Inst. C.E.

The existence of the salt-deposits of Stassfurt has been known for a very long period in the form of brine-springs, which were formerly worked for salt by the Ducal Government of Anhalt, and subsequently to 1796 by that of Prussia. The works were stopped in 1860, as the weak brine could no longer bear the competition with the stronger springs discovered by the deep borings in the Thuringian basin. In 1839 a boring was commenced which in 1843 reached the top of a salt deposit at 840 feet depth. A further depth of 1,066 feet was then bored through rock-salt without proving its total thickness, the work being stopped at 1,906 feet deep.

The result of this experiment was entirely unforeseen; the brine obtained, instead of being saturated with common salt, as was expected, proved to be a solution rich in potash and magnesian chlorides. As, however, the opinion of Dr. Karsten and Professor Maschand pointed to the probability of these more soluble salts being confined to the upper portion of the deposit, while purer rock-salt might be expected below, the Prussian Government decided upon sinking for the latter, and two shafts were begun in 1852 at Stassfurt, which in five years reached the salt-bed at 1,082 feet, after passing through the potash and magnesia salts between the depths of 840 feet and 918 feet.

The success of this sinking led to the opening of several other mines which were completed in the following order:—Leopoldshall, belonging to the Government of the Duchy of Anhalt, in 1862; Douglashall, belonging to the Consolidated Alkali Company of Westeregeln, in 1875; Neu-Stassfurt at Löderburg, near Stassfurt, in 1877; the Aschersleben Potash Works at Aschersleben, in 1873; and the Ludwig II. shaft at Stassfurt. The last-mentioned sinking, which is east of the original mine, was abandoned on reaching rock-salt in 1877, but after the new discovery had been made at Aschersleben it was reopened, and on further sinking, a deposit of the normal thickness of the potash-salt bed of the district was discovered in 1884.

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[1] This paper is mainly founded on a work by Dr. Precht, entitled "Die Salz industrie von Stassfurt," supplemented by information furnished by the Stassfurt syndicate for the Antwerp Exhibition, and by observations of the Author in October, 1885.

The preservation of these large quantities of the more soluble and deliquescent constituents of the original sea-water from which the salt of the district was derived, is attributed to evaporation going on under ordinary atmospheric conditions in an enclosed basin, communicating with the open sea by a strait or channel, which, while permitting the influx of sea-water, was too shallow to allow of any outward flow of the concentrated mother-liquors. By evaporation during the warmer months of the year, chloride of sodium was deposited in layers three or four inches thick; while in the colder weather sulphate of calcium (anhydrite) separated from the freshly-introduced sea-water; and in this way the alternations of the two minerals produced a structure analogous to the lines of growth, or annual rings, in tree-stems. The thickness of these rings of anhydrite averages about 7 millimetres (0·276 inch), and its proportion to the salt is about 8 per cent. Under these conditions, the 2,952 feet thickness of the salt bed of the Magdeburg-Halberstadt basin represent a period of growth of nine thousand nine hundred years.

Lately the full thickness of the Stassfurt salt-bed has been found in a boring near Unseberg. The older deposit was struck at 262 feet, and entirely penetrated at 4,101 feet. The dip varies from 35° to 45°, giving a horizontal thickness of 900 metres (2,952 feet). The rocks immediately below were anhydrite, black shale, and bituminous limestone; at 4,232 feet salt was again found, and at 4,243 feet the boring was stopped in grey anhydrite.

The soluble salts accumulated in the water of the basin after the deposit of the main bed of rock-salt, and the cutting off of fresh supplies of sea-water by the silting up of the channel were by continued evaporation brought up to the point of separation, which took place in several different ways. At first the anhydrite strings in the rock were replaced by polyhalite (a hydrated sulphate of potash, lime, and magnesia), producing the so-called "polyhalite region," which is about 60 metres thick, and is followed by the "kieserite region," in which the predominating mineral rock-salt is mixed with kieserite and carnallite, the proportion of the latter increasing upwards until in the "carnallite region" it forms the principal constituent of a workable bed. The carnallite, a double chloride of potassium and magnesia ($KClMg\ Cl_2 6H_2O$) is 82 feet thick, and forms the roof of the entire salt deposit, but it is so much mixed with other minerals that the average composition of the mass may be taken as: carnallite, 55; rock-salt, 26; kieserite, 17; and anhydrite and clay, 2 per cent. It also contains some boracite, interspersed in modules, which are picked out by hand.

When perfectly pure, carnallite is white, but generally it is coloured either grey or black by organic matter, or various shades of red by ferric oxide. It contains chloride of magnesium, 34·5; chloride of potassium, 26·76; and water 38·74 per cent.; is very deliquescent in moist air; readily soluble in water, and is produced from solutions of chloride of potassium, and chloride of magnesium, when at least 25 per cent. of the latter salt is present.

Kieserite, which is a constant associate of carnallite, is a monohydrated magnesium sulphate ($Mg\,SO_4\,H_2\,O$), containing magnesium sulphate, 87·1, and water 12·9 per cent. It is an amorphous translucent substance, white or grey, of specific gravity 2·517, which on exposure absorbs water from the air and changes into epsomite. Its formation is due to the hygroscopic character of the magnesium chloride in the mother liquors, which prevents the sulphate of the same base from combining with the necessary quantity of water, seven molecules, to form Epsom salts.

The carnallite deposit has been secured against the solvent action of subsequent overflows of water by an impermeable covering about 26 feet thick, consisting of saliferous clay, with gypsum at the bottom, and containing about 50 per cent. of carbonate of magnesium at the top. It also contains free alumina and magnesia. The saliferous clay is followed by a thickness of from 40 to 90 metres (131 to 295 feet) of anhydrite, forming the base of a newer rock-salt deposit, varying from 40 to 120 metres in thickness—which, however, is not continuous, but only occurs in the deeper hollows of the anhydrite. This newer salt is supposed to have been derived from the action of atmospheric water upon the salts of the earlier polyhalite deposit during a period of elevation. It is worked at Neu-Stassfurt, where it was first discovered, and is much purer than the older bed, containing 98 per cent. of chloride of sodium.

The close of the salt-period was marked by the deposit of a great thickness of red sandstones and coarsely oolitic (*rogenstein*) dolomite, which have subsequently been thrown into a series of ridges and furrows, along an axis running about S.E.–N.W.

The carnallite region has been traced along the strike of the formation for about 22 miles between Westeregeln and Aschersleben; the maximum breadth is about 12½ miles.

By the infiltration of surface-waters at the crest of the principal ridge, the shallow-lying portions of the old rock-salt bed have been removed down to 70 metres below the surface, while the more soluble carnallite has been destroyed to a greater depth, and is not found in workable quantity above 650 feet. Changes have also been caused by these subsequent infiltrations, even where complete solution has not been effected, resulting in the formation of several new compounds. Where the action has been of short duration only chloride of magnesium has been removed from the carnallite, leaving the so-called hard salt a mixture of magnesium, chloride, and sulphate, and chloride of potassium; but where it has been longer continued the magnesium sulphate becoming soluble, by absorption of water reacted upon the potassium chloride, forming potash-magnesium sulphate and magnesium chloride, which combined together forming the so-called kainite. This latter change is the more general, the hard salt being only found at a few points in the Leopoldshall and Douglashall mines, while kainite is found pretty constantly over the whole district, and although considerably less abundant than carnallite, it is more valuable, as it can be used as a fertilizer without further preparation. When perfectly pure it is white, and of the composition ($K_2 SO_4 Mg SO_4 Mg Cl_2 6 H_2 O$), but in bulk it is much mixed with other minerals, so that the average composition of the product when prepared for sale is as follows: Potassium sulphate, 24; magnesium sulphate, 16·5; magnesium chloride, 13; water, 14; chloride of sodium, 31; clay and gypsum, 1·5 per cent.

A large number of other minerals have been found at Stassfurt, but many of these are rare, and only of scientific interest. The complete list is as follows:—

Minerals of primary origin:—
Rock-salt, $Na Cl$. Anhydrite, $Ca SO_4$.
Polyhalite, $K_2 SO_4 Mg SO_4 2 Ca SO_4 2 H_2 O$.
Kieserite, $Mg SO_4 H_2 O$. Carnallite, $K Cl Mg Cl 6 H_2 O$.
Boracite ($2 (Mg 3 B_8 O_{15}) Mg Cl_2$). Douglasite, $2 K Cl Fe Cl_2 2 H_2 O$.

Minerals of secondary origin:—
Kainite ($K_2 SO_4 Mg SO_4 Mg Cl_2 6 H_2 O$).
Sylvine, $K_2 Cl$. Tachhydrite ($Ca Cl_2 2 Mg Cl_2 12 H_2O$).
Bischofite ($Mg Cl_2 6 H_2O$). Krugite ($K_2 SO_4 Mg SO_4 4 Ca SO_4 2 H_2O$).
Reichardite Epsomite ($Mg SO_4 7 H_2O$).
Glauberite ($Ca SO_4 Na 2 SO_4$.) Astrakanite ($Na_2 SO_4 Mg SO_4 4 H_2O$).
Pinnoite ($Mg B_2 O_4 3 H_2O$).

Douglasite, which is only found in traces, is remarkable for the property of decomposing its combined water in the presence of magnesia, producing ferric oxide, chloride of potassium, chloride of magnesium, and hydrogen. The latter gas was given off in large quantity by the carnallite bed when it was first cut, and burnt with a long flame for several months.[1]

The potash-salts are worked at present from eleven different shafts, which number will be increased in a few years by five more that have already been sunk. Of the former number, three belong to the Prussian Government; two to that of Anhalt; two to the Neu-Stassfurt Company in Löderburg; two to the Consolidated Alkali works of Westeregeln; two to the Aschersleben salt works, and one to the Ludwig II. Company. Their depth varies from 300 to 500 metres (984 to 1,640 feet). The two shafts in Neu Stassfurt, the Achenbach shaft at Stassfurt, and the Schmidtmann shaft at Aschersleben are circular, from 4½ to 5½ metres in diameter, and lined with masonry; the remainder are rectangular and timbered. At Aschersleben and Schönebeck more water has been encountered in sinking than could be kept back by walling in cement, so that partial iron tubbing is used. The last-mentioned shaft is still sinking, and is now 300 metres deep.

The working of the potash-salts is effected by means of levels about 65 feet vertically apart, which are connected with the shafts by cross-cuts. The main levels follow the strike of the bed, and are, in some of the mines, several miles in length already. In the older method of working, drifts 30 feet wide were driven at right angles to the main level, leaving a pillar of 20 feet on either side. The salt was excavated to a height varying from 26 to 50 feet according to the distance between the levels, a thickness of 20 to 26 feet being left to support the ground overhead. The length of the stalls varies with the angle of the bed, which is nearly horizontal in Aschersleben, but dips 12° in Aschersleben, and 50° to 60° in Neu-Stassfurt. At Westeregeln the Carnallite bed is nearly vertical. About 40 per cent. of the whole of the deposit is won by the above method, while 60 per cent. remains in pillars. It was supposed that this would have given sufficient support to the ground, but the results of twenty years' working show considerable subsidence in the upper levels, and it has therefore been decided in future to remove the whole of the mineral, and to fill up the excavation with salt specially quarried in the lower bed. This order extends to both public and private mines, the old excavations are to be entirely filled during the next five years. It is expected that the extra cost of working will be covered by the larger yield due to the saving of the pillars. In the upper salt-bed, the working is by stalls 22 metres wide with 15-metre pillars, which are found to give sufficient security.

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[1] E. Pfeiffer considers this to have been mainly hydrocarbon gas derived from the decomposition of included organic matter (Zeitschrift für Berg- Hütten und salinen-wesen, xxxiii. p. 77).

The salt is got entirely by boring and blasting, Lisbet's rotary borer being largely used as well as machines driven by compressed air. A weak powder made of nitrate of soda and charcoal is principally used ; dynamite is unsuitable on account of its rapid action. About 1 kilogram of powder is used per cubic metre of carnallite raised, of which about $\frac{4}{5}$ are required in the preliminary driving and the remainder in the stalls. The quantity broken by one man in the shift of eight hours is about 5 tons when driving and 30 tons when working in the stalls.

The mineral, which is, where possible, picked over in the mine in order to separate the valuable contents, carnallite and kainite from kieserite and rock salt, is loaded in tram wagons carrying from 12 to 16 cwts. which are drawn along the levels to the shaft. This is done by men or horses, except at Neu-Stassfurt, where an electrical railway by Siemens and Halske has been at work for some months past. The arrangements are similar to those at Zaukeroda,[1] the return conductors being carried upon cross-timbers about 7 feet above the floor, the levels being of great height. The lengths of the lines at work are 610 yards and 492 yards respectively. In February 1884, an average quantity of 760 tons was carried daily at a total cost of £2 19*s*. This shows a saving of 39 per cent. on the cost of the hand-labour previously used.

At the same mine the drawing is conducted in cages with safety-catches. The horizontal direct-acting winding-engines having 30-inch cylinders and 59-inch stroke, with a drum of 16·4 feet diameter, draw a gross load of 5 tons (or 2 tons net), from the bottom of the shaft (1,170 feet) in 40 or 50 seconds.

The bulk of the carnallite (95 per cent.) is delivered to the chemical works in lumps as raised, the remaining 5 per cent. being ground for the purpose of sampling, as the price is determined by the result of the assay for potash. Kainite and rock-salt are ground before delivery. Both conical mills, resembling coffee-mills, and French burr-stones are used, the former for breaking down, and the latter for grinding to the required fineness, which varies from 0·5 to 3 millimetres (0·0195 to 0·118 inch) for the diameter of a grain. The grinding-plant at the different mines includes forty-three conical mills and fifty-two pairs of stones, capable of treating 1,700 tons daily, besides four Vapart crushers, with stone-breakers at Neu-Stassfurt, which are principally used for crushing kainite.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxiii. p. 474.

The production of the mines has, apart from small variations, steadily increased from year to year, the average annual produce being, in tons of 2,204 lbs. :—

Period.	Rock Salt.	Carnallite and Kieserite.	Kainite.	Boracite.
1861–1865 . . .	44·494	29·603	..	2·9
1866–1870 . . .	58·937	118·699	11·689	16
1871–1875 . . .	64·233	433·859	18·039	17
1876–1880 . . .	96·856	643·363	55·773	84
1884[1]	160·109	585·016	203·120	153

About four thousand hands are employed in the productive salt-mines of the district, which are equipped with seventy-six boilers and fifty steam-engines. Thirteen of these are employed in winding and the remainder in the different services of pumping, ventilating, grinding, &c.

The first factory for the production of chloride of potassium, established at Stassfurt by Mr. A. Frank in 1861 was quickly followed by many others, so that in 1864 there were twelve at work, which number was in excess of the requirements of the time. Subsequently, however, the demand has sufficiently increased, so that at the present time some twenty establishments, twelve belonging to private firms and eight to mining and joint-stock companies, are in operation.

The process consists essentially in dissolving carnallite in water to saturation at the boiling temperature, which solution on cooling deposits chloride of potassium, while chloride of magnesium remains dissolved. This apparently simple operation is, however, attended with certain necessary difficulties, and has only been gradually brought to the present working condition, after numerous modifications and improvements. The general stages of the process are as follow :—

1. *Solution of the crude Salt.*—The rough potash-salts, after being crushed or ground, are delivered by elevators to the high-level dissolving-boilers, which are cylinders of 12 cubic metres (424 cubic

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[1] These figures are from the statistical return. Zeitschrift für Berg-Hütten- und salinen-wesen, 1885, p. 25. The report of the Syndicate gives the production of rock-salt at 180,000 tons in 1884.

feet) capacity, containing boiling solutions of choride of magnesium resulting from previous operations. High-pressure steam is then admitted, which dissolves out potassium and magnesium chlorides, while rock-salt and magnesium sulphate are not attacked. The resulting hot liquor of 1·32 specific gravity is passed into pans to allow the suspended matters, kieserite and clay, to deposit, and when clarified into the crystallizer where, after two or three days' cooling, a mixture of chloride of potassium and common salt is deposited. In some works the hot liquors are diluted with water, which keeps back the latter salt, so that almost pure chloride of potassium crystallizes out.

2. *Concentration of Mother Liquors.*—In order to recover the potash still remaining in the first mother liquor, it is concentrated by evaporation to such a point that a mixed salt, exactly similar in composition to carnallite, separates, leaving only 0·5 to 1 per cent. of the original contents of chloride of potassium in the waste liquor. This artificial carnallite is dissolved in hot water, and on cooling chloride of potassium deposits, as in the treatment of the crude salt.

3. *Purification of the Potassium Chloride.*—The product of the first crystallization is washed with water at the ordinary temperature of the air, which removes magnesium choride wholly and sodium choride partially, and then dried either in a calcining-furnace or in a steam-heated stove, when it is ready for market. It is largely used for the production of carbonate of potash, by Lablanc's process, and other potash-salts, and for the manufacture of saltpetre from nitrate of soda. For the latter purpose 80 per cent. choride of potassium is generally used, but for the former the perfectly pure salt is preferable.

The second mother liquors and washings of the different crystallizations containing chlorides of potassium, soda, and magnesium, make up the liquor used in dissolving the crude salt. From 75 to 85 per cent. of the total chloride of potassium in the crude salt is recovered, the remainder being variously distributed through the end liquor and the residues of the dissolving and necessary operations. The amount in the mud deposited from the first solution varies very considerably, and in some works it is sufficiently high to give a product with 18 to 24 per cent. of chloride of potassium when calcined. This is sold as a fertilizer. Taking the loss at 20 per cent., 6·25 tons of crude salt are required to produce 1 ton of 80 per cent. chloride of potassium. In order to treat 50 tons of crude salt daily a crystallizing capacity of 400 cubic metres is required. The total contents of the crystallizers in the whole of the works is 32,000 cubic metres. In 1884 about 120,000 tons of chloride of potassium of the different guaranteed assays of 80, 85, 88, 90, 95, 96, 97, and 98 per cent. were produced and sold.

Kieserite and Glauber's-salt are the principal residual products of the potash manufacture. The first is obtained by dissolving out the chloride of sodium from the rough salt residues, whereby the less soluble kieserite subsides as a fine mud, when exposed to the air for a few hours, which absorbs water and solidifies. It is sold in blocks from 40 to 70 lbs. weight at a guaranteed assay of 55 per cent. of sulphate of magnesium. It may be used as an absorbent of ammonia when finely ground and mixed with stable litter, as well as for the manufacture of Epsom salts.

The production of Glauber's-salt, which is only possible in the winter time, depends upon the reaction of chloride of sodium upon sulphate of magnesium when in solution at temperatures below the freezing-points of water producing sulphate of sodium and chloride of magnesium. For this purpose the entire residues of the manufacture are dissolved in water and the concentrated solution is exposed to the air in shallow wooden trays during the cold winter season. In moderately cold weather the crystallization is completed in one night. At the present time cooling-pans, having a surface of 10,000 square metres (107,642 square feet) are in use, which suffice for the treatment of the residue from 150,000 tons of carnallite in a moderately cold winter. The Stassfurt Glauber's-salt is perfectly neutral, and as nearly as possible free from iron-salts.

Kainite is mostly sold as such, about 158,000 tons having been disposed of in 1884. A large proportion of this was exported to North America. When ground for sale it is guaranteed to contain 23 per cent. of chloride of potassium. The remaining 44,000 tons were treated at three of the chemical works to remove the chlorides, each using a different process. This gives a double sulphate of potash and magnesia, which is in considerable demand as a manure for tobacco, vines, and various fruit-crops. It contains 50 per cent. of sulphate of potassium and only 2 per cent. of chlorine.

The utilization of the enormous quantities of residual chloride of magnesium is a still unsolved problem. To a small extent it is concentrated by evaporation, and heated sufficiently to drive off some of the water of crystallization; the product, when run into casks and solidified, is sold as fused chloride of magnesium. It contains about 50 per cent. of the latter salt, from 1 to 2 per cent. of potash-, soda-, and lime-salts, the remainder being water.

The chief uses are for sizing purposes in textile manufactories, and, in a lesser degree, for freezing-mixtures, for producing the circulating fluid in freezing-machines; for a disinfectant, and for the production of hydrochloric acid and magnesia by Ramdohr's process. It has also been used to some extent as a source of magnesia for use in converter linings for the basic process of steel-making, by Closson's process,[1] but this has now been abandoned. A very much larger proportion of the end liquor is treated with chlorine to recover the small proportion (0·2 per cent.) of bromine contained. This is done either by distilling the concentrated hot solution of chloride of magnesium with peroxide of manganese and sulphuric acid in sandstone stills, or by a new method introduced at Leopoldshall, in which a current of chlorine is passed continuously through the liquor. At the present time about two-thirds of the total chloride of magnesium waste-liquors are thus treated, and about 400 tons of bromine are saved annually.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxii. p. 403.

OBITUARY.

HENRI TRESCA[1] was born at Dunkirk on the 12th of October, 1814, and died in Paris on the 21st of June, 1885, in his seventy-first year. In many respects a remarkable man, his chief claims to recognition lie in his researches on the "Flow of Solids," a branch of scientific inquiry of which he may be said to be the founder, and to the prosecution of which he brought all the resources of a singularly powerful and active mind.

Tresca's life-work may be divided into three parts: first, the period of preparation, during which, while engaged with success in the ordinary pursuits of an engineer, he was assimilating and storing up the results of experiment and observation, which youthful enthusiasm and love of work enabled him to accumulate to an extent simply marvellous in its amount and far-reaching. Second, his long association with General Arthur Morin, at the Conservatoire des Arts et Métiers, the result apparently of accident, but towards which his previous training would seem to have been unconsciously directed. Third, his position as an independent investigator and man of science.

Of the first of these divisions it will suffice to speak in general terms. The future brilliant Professor pursued, at the Lycée Louis le Grand, and afterwards at the École Polytechnique—the ordinary curriculum of a young French collegian—with credit, but without betokening extraordinary promise, excepting always that he manifested astonishing powers of rapidly learning his lessons. In France, the student-period usually endures longer than in England, and it is no uncommon thing to find a bearded man of twenty-four still submitting to the restraint of a discipline almost military; but by overtaking his "courses," Tresca was enabled to enter the École des Ponts et Chaussées, in 1835, in his twenty-first year. He would doubtless have begun his career at the bottom of the ladder as an engineer in the government corps of the "Ponts et Chaussées;" but a long illness determined him to abandon his prospects of advancement in the state service, and to become an *Ingénieur-civil.*[1]

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[1] More detailed accounts of Professor Tresca's life and work will be found in the Mémoires de la Société des Ingénieurs-civils, Paris, July 1885, p. 130.

He was employed on various applications of engineering to the industrial arts for about fifteen years, when he was nominated as principal inspector of French products contributed to the Great Exhibition of 1851. In this capacity he made the acquaintance of General, then Colonel, Morin, professor of applied mechanics in the Conservatoire des Arts et Métiers, and director of that establishment. Meeting one day in the gallery of machinery in motion of the Exhibition in Hyde Park, they exchanged ideas as to the advantages which would accrue from a permanent institution of that character in Paris. Morin, struck by the sense and sagacity of his companion, offered him on the spot the position of engineer to the Conservatoire, a post which did not exist, but which he knew he could easily obtain authority to create, and which his knowledge of men told him intuitively Tresca was peculiarly fitted for. It proved, in fact, the realization of a happy thought of Morin's, for a laboratory of industrial-mechanics, the idea of which he had conceived many years previously; but which had awaited for its accomplishment the advent of an auxiliary, capable, assiduous, and enthusiastic. Tresca was the man.

Tresca had now reached the second and active period of his career. His yearning for some means of showing the stuff he was made of had been gratified. He had met in Morin a kindred spirit, and begun an ardent friendship which only death would interrupt.[2] He was in the midst of, to him, a fairyland of mechanical and physical appliances, most of which he was eager to test, to show wherein they were deficient, and probably to suggest the improvement thereof; above all to drag them from their obscurity, dissect, and make them a means of advancing mechanical knowledge.

To appreciate rightly the influence of Tresca's work on the mechanical arts in France, the reverence of his countrymen for centralization must be taken into account. For long years the Conservatoire des Arts et Métiers has been an object of veneration for French mechanics all over the country, and the general public largely share the feeling. The sightseers who visit the curious nave of the church of St. Martin des Champs, in which is installed the machinery in motion annexed to the Conservatoire, notice only that which excites their curiosity. They are, perhaps, unconscious that in this place were carried out fundamental experiments and innumerable tests of machines and apparatus, all of which have directly or indirectly benefited themselves. It requires therefore the consideration of the improvements that have been made in all classes of machinery during the past thirty-five years, and the reflection that the germs of many of the most important are to be found in the silent, unobtrusive work done here by him, or under his supervision, in order to estimate Tresca's labours at their true value.

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[1] "Ingénieur-civil" has a much more restricted meaning than civil engineer. In France the man who undertakes what we know as works of civil engineering (excepting railways) is a government official—Ingénieur des Ponts et Chaussées. The Ingénieur-civil is a free-lance, whose equivalent title in English would rather be "Industrial" engineer. The alma mater of the former is the École des Ponts et Chaussées; of the latter the École centrale des Arts et Manufactures, and the distinction is as sharply maintained as that between military and civil engineers in England.

[2] The memoir of General Morin, Minutes of Proceedings Inst. C.E., vol. lxii. p. 349, was written by Professor Tresca.

The experiments instituted at the Conservatoire, and executed with a skill and conscientiousness above all suspicion, have played a considerable part in the evolution of new inventions, whilst having regard to the older ones they were intended to supersede. By the aid, principally of instruments devised by General Morin, all the new motors and machines deposited at the Conservatoire were tested. None escaped this ordeal, and when the results were translated into figures no one ever contested their exactitude.

In a notice compiled in 1868, when he was a candidate for the Académie des Sciences, Tresca says: "My labours in connection with applied mechanics mainly consist of scientific and experimental researches concerning either the properties of materials or the mode of working, and the results yielded by the principal elements of industry. Numerous as these experiments have been, they were not made with any idea of creating a title to the consideration of the Academy, but were rather a compulsory adjunct of my lectures on applied mechanics at the Conservatoire des Arts et Métiers."[1] This record contains a detailed and precise classification of Tresca's various experiments. With its supplementary notice[2] it extends to 92 quarto pages of what, if his life-work had been one continuous lecture, would constitute a syllabus of his labours. The classification is as follows:—

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[1] "Notice sur les Travaux Scientifiques de M. Tresca." Paris, 1868.

[2] "Notice supplémentaire sur les Travaux Scientifiques de M. Tresca, Professeur de Mécanique au Conservatoire des Arts et Métiers et à l'École Centrale des Arts et Manufactures." Paris, 1871.

I.—*Strength of Materials.*

Crushing-strength; tensile-strength; resistance to tension; bending-strength; tests of construction.

II.—*Flow of Solids and its Application.*

III.—*Motive-Power and Motors.*

Animal motors; hydraulic accumulators; steam-boilers; steam-engines; gas- and atmospheric-engines.

IV.—*Mechanical Equivalent of Heat.*

V.—*Applications of Motive-Power.*

Traction of vehicles; hydraulic elevators; trimming of loads (employment of gearing for); agricultural machines; industrial machines.

VI.—*Various Mechanical and Physical Problems.*

VII.—*Weights and Measures.*

When it is considered that these experiments were but ancillary to his professorial work, and that each item includes the testing and description of unnumbered complicated machines, some conception may be formed of Tresca's powers of application. It will be noted that the section "Flow of Solids" makes no show above its fellows, but the description of his method of experimenting, and the materials chosen, occupy seven closely printed quarto pages. It was the substance of his constant thoughts from the time it first occurred to him in 1862, when observing the behaviour of a piece of iron being punched, to the day of his death, and was doubtless the most influential factor in obtaining his admittance to the Académie des Sciences.

Space does not admit of more than a passing reference to Tresca's labours in this direction. In constructing his theory of the flow of solids he relied mostly on synthetical methods; nothing was taken for granted. Slowly and laboriously he built up his theory from countless experiments on specimens of various metals, hard and soft; and not till after long years of patient inquiry did he feel justified in formulating his conclusions as follow:—

1. That pressure is transmitted in solids as in liquids, in every direction, with loss of pressure from one point to another.

2. That the deformations shown in solid bodies may be likened to the result of a flow occurring in the direction of least resistance.

3. That in every case where deformation can only occur by the passage of matter through an orifice there is formed a prismatic jet, adapting itself, like a liquid vein, to this orifice, and in which

must be recognised the mode of repartition of the matter proceeding from every part of the original solid.

4. That it is possible to express by suitable equations the surface into which will be transformed a given plane, or a cylinder, considered in respect of its primitive solid state.

5. That the same processes of calculation lead, in many cases, to the determination of the trajectory of a particular molecule, considered either as part of a solid or of a fluid.

6. That by means of this determination the domain of mechanical science is widened, and that as regards the strength of material, it can no longer be limited to considering the deformations resulting from the transmission of efforts in the elastic period alone, but can render account of all the circumstances accompanying any given deformation.

The records of most of these experiments are to be found in the "Annales du Conservatoire des Arts et Métiers," of which publication Tresca was the principal founder, but there are many scattered papers appearing in the Comptes Rendus of the Académie des Sciences, the "Bulletin de la Société d'Encouragement pour l'Industrie nationale" and other similar periodicals.

The Conservatoire des Arts et Métiers was an established Institution before Tresca's time, and it would be unjust to the memory of others, notably his dearest friend General Morin, to claim for him more than that he contributed to uphold its world-wide reputation; but his contribution was full measure, well beaten down. In the history of this latter great organization the name of Tresca will be associated with those of Charles Dupin and Arthur Morin as one of a triad who have done most to make it what it is. Besides his functions of Professor of Applied Mechanism and Sub-director of the Conservatoire, Tresca held numerous other official or quasi-official appointments. He was Professor of Applied Mechanics at the École Centrale des Arts et Manufactures, and Matriculation-Examiner at the same school, member of the Committee on the Mechanic Arts of the Société d'Encouragement, Honorary President of the Société des Ingénieurs-Civils, a life-honour created specially to mark the Society's sense of their indebtedness to one who had thrice held the position of President. In 1872 he was elected a member of the Académie des Sciences in the Mechanical Section, in succession to the veteran Combes. He was also "of" the Société Nationale d'Agriculture, and was entitled to place after his name a string of the mysterious symbols dear to Frenchmen as representing so many coveted orders. It has been mentioned that as a youth he was a quick learner. This faculty he retained unabated through life, and a remarkable illustration of it occurred during the International Electric Exhibition held at Paris in 1881, when he was nearly seventy years of age. Tresca previously knew little of electricity, but in the fortnight that the Exhibition remained open he made such use of his time that it was remarked, "He entered it a novice, to leave it a master." That the epigram was justified may be inferred from the fact that he speedily became a recognized authority on the subject, especially that part of it connected with the electric transmission of energy, as to which his views were respectfully deferred to by many leading electricians.[1]

Of Tresca's private character little need be said. He was a man of genius, and had the foibles and eccentricities attaching thereto. He appears to have found in Morin one on whom to lavish a more than brotherly affection, and, outside of his family, he was concerned little in minor friendships. His own great powers made him somewhat impatient of the duller perceptions of the multitude, and he was considered very strict in the examination of his pupils. It was generally expected that he would be appointed Director of the Conservatoire des Arts et Métiers on General Morin's death, but considerations of policy induced the Government to confer that distinguished office on Mr. Hervé Mangon. This was undoubtedly a great disappointment to Tresca, and to some extent embittered the remaining years of his life. Apart from this, he was respected if not loved, and a vast crowd accompanied his body to the Montmartre Cemetery, braving the heat of a tropical summer's day to listen bareheaded to the funeral orations pronounced over his remains by no less than five leading representatives of different scientific bodies to which he had belonged.

Professor Tresca was elected an Honorary Member of this Institution on the 4th of February, 1879, on the proposal of the chairman of the meeting, Mr. W. H. Barlow, F.R.S., his nomination, in accordance with established usage in such cases, being seconded by all the members of council then present.

FREDERICK BARRY, a younger son of Mr. Robert Barry, Chief Commissioner of Military Accounts in Ireland, was born in Dublin on the 15th of April, 1821, and was educated at Clifton, Bristol, and at Trinity College, Dublin.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxx. p. 455.

In 1841 he was articled to Mr. Tierney Clark, M. Inst. C.E., who had an established reputation as an engineer for the construction of suspension-bridges. In 1842 Mr. Barry accompanied him to Hungary, where he was employed in completing the works of the famous Buda-Pest suspension-bridge across the Danube. This memorial of English engineering is fully described in a work to be found in the Library of this Institution.[1]

Mr. Barry was entrusted by Mr. Clark in superintending various branches of that great work, and was also engaged in charge of the Danube and Theiss Canal. On the completion of his pupilage he returned home, and was for a year Resident Engineer on the Blackburn and Bolton Railway. In 1847 he was employed by the Board of Works in Ireland to report on, and to make plans and estimates for the drainage of the river Suck, an affluent of the Shannon, but by reason of the non-assent of the adjoining landowners to the expenditure the works were not carried out. Mr. Barry was subsequently employed by the Board in making plans and carrying out and constructing important drainage-works in twenty-two districts in the county of Mayo. He resigned his functions under the Board in 1855 in order to start in independent practice as a civil engineer, and he was so engaged for the remainder of his career. His practice was largely in connection with railways. He acted, in conjunction with Mr. (now Sir John) Fowler, Past-President, in the construction of the Great Northern and Western (Ireland) Railway, a total length of about 105 miles, now forming part of the Midland Great Western system, and as sole Engineer of the Castlebar and Ballina parts of the same system. He also acted as engineer to some drainage works near Berlin. Mr. Barry's last undertaking was the Sligo, Leitrim and Northern Counties Railway, the bill for which he promoted and carried through Parliament. This line, 40 miles long, connects Enniskillen with Sligo, crossing Lough Erne by an important bridge. Mr. Barry in 1877 allied himself with Col. A. L. Tottenham, M.P., one of the principal landowners concerned, and carried out a large portion of the works, including the bridge over Lough Erne, as engineer and contractor. His engagement with Col. Tottenham closed in 1880. His health shortly afterwards failed, and for upwards of two years before his death he

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[2] "An Account, with illustrations, of the Suspension Bridge across the river Danube," &c. By W. Tierney Clark, F.R.S.

was unable to engage in any professional work. He died on the 3rd of June, 1885.

Mr. Barry was elected an Associate on the 6th of April, 1852, and transferred to the class of Members on the 28th of April, 1857.

PETER DUCKWORTH BENNETT, the fourth child of William Bennett, was born at the village of Hawarden, near Chester, on the 5th of May, 1825. He was educated at the Hawarden Grammar-school. In 1839 he was articled to the Oak-Farm Iron Company, near Dudley, for seven years, and at the termination of his articles in 1846 became Manager of the Constructive Department of that Company.

In 1847 he entered the service of Messrs. Cochrane and Co., Woodside near Dudley, and in 1850 attained the position of chief of the drawing, estimating, and constructing offices. He left Woodside in 1852, and was appointed Chief Engineer at the works of Messrs. Fox, Henderson and Co., where he remained until 1853, when he commenced business on his own account at Spon Lane, as a mechanical engineer, being, until 1878, principally engaged with works for railways and gas companies. In 1878 his business was amalgamated with the Horsley Company Limited, Tipton, for which company Mr. Bennett acted as Managing Director and Engineer. He was Chairman of the Sandwell Park Colliery for some years, and was one of the chief promotors of the trial-sinking in 1869, which he carried out perseveringly until ultimately success was realized by the proving of coal at a depth of 400 yards.

Mr. Bennett was a Member of the Institution of Mechanical Engineers, and of the Iron and Steel Institute. He was a Director of the Birmingham Joint Stock Bank, a Justice of the Peace for the county of Stafford, a Member of the Council of the Birmingham and Midland Institute, and formerly for many years Chairman of the West Bromwich Commissioners. He took a lively interest in the building of the new Art Gallery in Birmingham, and lent a collection of ivory carvings and damascene work, which is on view at the present time. His death occurred from a deplorable accident on the 28th of November, 1885, during the visit of the Prince of Wales to Birmingham. He was elected a Member of this Institution on the 13th of January, 1885.

ROBERT HANDCOCK was a son of the Rev. Robert Handcock, a clergyman of the Church of Ireland, and cousin of Lord Castlemaine. He served his pupilage to Mr. W. R. Le Fanu, M. Inst. C.E., and was subsequently engaged as Resident Engineer for that gentleman on the Killarney Junction Railway, now part of the Great Southern and Western system. The long tunnel at Cork, forming part of this undertaking, was also under Mr. Handcock's supervision as Resident Engineer. In 1853 Mr. Handcock was appointed an Assistant-Engineer on the Bombay, Baroda, and Central India Railway, and was in due time promoted to the charge of a district on that line. In 1858 he left India for New South Wales, in which colony he spent the remainder of his life. Being of independent fortune he was free to regard his profession as a pleasant occupation rather than as a means of existence; but he did good work in the Australian Colonies in the direction of water-supply, the study of hydraulics having for him, in common with many of the most highly-gifted engineers, a peculiar fascination. Mr. Handcock was engaged on the survey for the Sydney water-supply in 1868, and in August of the following year was appointed a "Licensed Surveyor" of New South Wales, and employed under Mr. C. F. Adams, Surveyor-General, until April, 1876. At the latter date he was removed to the Staff and served in that capacity until his death, on the 13th of February, 1885. His excellent survey of Port Hacking first brought him under the notice of the Surveyor-General, and the plan he made to accompany Mr. W. Clark's report on the Sydney water-supply is still a standard one in the department.

Mr. Handcock was elected a Member of this Institution on the 1st of March, 1876.

JOHN TOWLERTON LEATHER, the eldest son of James Leather, colliery-proprietor of Beeston Park, Yorkshire, was born on the 30th of August, 1804. He served his articles with his uncle, George Leather, the Engineer of the Aire and Calder Navigation, of Goole Docks, and of other works, and entered on the practice of his profession at Sheffield in 1829.

In 1833 Mr. Leather was appointed Engineer of the Sheffield Waterworks, and in that capacity carried out the large impounding-reservoirs at Redmire's, and the distributing-reservoirs at Crooke's Moore, near Sheffield. During the progress of those works, Mr. John Fowler, Past-President Inst. C.E., became his pupil. In

1839 Mr. Leather, whilst retaining the appointment of Consulting Engineer to the Sheffield Waterworks, gave up the regular practice of his profession and commenced business as a contractor, in partnership with Mr. Waring, the father of the members of the present well-known firm of Waring Brothers, their first joint undertaking being a portion of the Midland Railway at Chesterfield, of which Mr. George Stephenson, then residing at Tapton House, was the Engineer. Subsequently those gentlemen executed the Chester and Crewe section of the London and North-Western system.

The partnership with Mr. Waring having been dissolved, Mr. Leather then undertook on his own account the Tadcaster and York Railway, which was abandoned by the Company when about half completed, and from 1847 to 1850 he constructed the Erewash Valley line of the Midland system. In 1854 and 1855 Mr. Leather was engaged in carrying out the River Nene improvement-works between Wisbech and Peterborough, under Mr. James Meadows Rendel, Past-President Inst. C.E., as engineer: these works, which were estimated to cost £120,000, were abandoned for want of funds in 1855. About this time, and under the same engineer, Mr. Leather successfully put in the foundations of the Suspension Bridge at Inverness, under very difficult circumstances, and after the failure of two other contractors to carry out the work.

Sir John Hawkshaw having been called upon to grapple with the disastrous inundation of the Middle Level, which occurred in 1862, secured the aid of Mr. Leather and his staff, and the work of constructing the dam and siphons, fully described in Sir John Hawkshaw's Paper,[1] was energetically and successfully carried through. The emergency both of this work and of that of the Inverness Bridge rendered it impracticable to enter into any contracts, and the fact that they were entrusted to Mr. Leather, by their respective engineers under these circumstances, is a striking testimony, therefore, to the confidence reposed both in his skill, his energy, and his integrity.

But Mr. Leather's name and reputation will be chiefly associated with the three great national undertakings, for two of which, the Portland Breakwater and the Sea-Forts at Spithead, he was the sole contractor; in the third, the extension of H. M. Dockyard at Portsmouth, he was associated with the late Mr. George Smith, of Pimlico.

The construction of the Portland Breakwater, for which some preliminary works had already been executed, was entrusted to Mr. Leather in 1849, Mr. James Meadows Rendel, Past-President Inst. C.E., being the Engineer-in-Chief, and Mr. (now Sir John) Coode, Vice-President, the Resident-Engineer, until the death of the former, in 1856, when the works were completed under the sole charge of Sir John Coode. An undertaking of this magnitude necessarily encountered many difficulties from the elements, but the skill and prudence with which they were met, and the efficiency of the appliances provided for the purpose, were the means of preventing all very serious accidents but one or two in the earlier stages of the work.

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[1] Minutes of Proceedings Inst. C.E. vol. xxii. p. 497.

In 1861 Mr. Leather entered into the contract with the War Office, under Sir John Hawkshaw as engineer, for the construction of the foundations of the Sea-Forts at Spithead, Simultaneously with this contract he built the Gilkicker and St. Helen Forts, directly under the supervision of the Royal Engineer Department; and subsequently the superstructures of the Spithead Forts, also under Royal Engineer supervision. This contract was completed in 1872. In 1867 Mr. Leather and Mr. George Smith, entered upon the work of extending the Portsmouth Dockyard, an undertaking involving the expenditure of over £2,000,000 of public money. On account of the depth of mud and other conditions of the site, the cofferdams required were necessarily of a very expensive and substantial character; these, however, and every other appliance requisite for the efficient execution of the work, were provided with a liberality and completeness which has rarely been equalled, and which made an inspection of these works during the course of their execution one of the most noticeable engineering features of the period, and attracted numerous visits from engineers and other scientific persons, both English and foreign. This work and the appliances employed have been described in Papers by Mr. C. Colson, Assoc. M. Inst. C.E., of the Dockyard Engineers Staff, and Mr. C. H. Meyer, Assoc. M. Inst. C.E., who assisted the contractors.[1]

On the completion of this contract, about the year 1877, Mr. Leather withdrew from active business life, and occupied himself with local affairs and in the management and improvement of his estates in Yorkshire and Northumberland, of which latter county he was a magistrate and deputy-lieutenant, and for which he served as High Sheriff in 1875. Mr. Leather, who was twice married, died at Leventhorpe Hall, near Leeds, on the 6th of June, 1885.

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[1] Minutes of Proceedings Inst. C.E. vol. lxiv p. 118

In the course of his career as a contractor, Mr. Leather's position and reputation naturally led to his being made many offers for the carrying out of large works abroad, but these proposals never got beyond the initiatory stage. His advice was frequently sought on engineering and other kindred questions, and his well-known prudence and tried experience enabled him to be of service in this way on many occasions. The confidence reposed by Mr. Leather in his staff, and the freedom with which they were thus enabled to grapple with the many sudden emergencies inseparable from sea-works, were also important factors of his success in these undertakings.

Personally Mr. Leather was extremely courteous and fair-minded, and he was also one of the most modest and unostentatious of men, shrinking from any kind of publicity in connection even with such large undertakings as fell to his lot to carry through; combined with this was a strong love for thoroughness and for work of the best character, which is clearly stamped upon the enterprises by which his name will be long remembered.

Mr. Leather was one of the oldest Members of the Institution, having been elected on the 23rd of February, 1836, and having all but completed a membership of half a century.

FRANCIS MATHEW [1] was the son of Mr. Francis James Mathew of Rock View House, Co. Tipperary, and nephew of the celebrated Father Mathew. He commenced his career in 1847 as a pupil under Mr. Joseph Burke, senior District Engineer, on the establishment of Sir John Macneill, M. Inst. C.E. After four years with Mr. Burke, during which time he was engaged in the construction of the Dundalk and Enniskillen and the Waterford and Kilkenny Railways, Mr. Mathew became an assistant engineer on the establishment of Mr. William Dargan, the well-known railway-contractor. He remained in this service for six years, during which time he held the position of Engineer on the Limerick and Foynes, and the Limerick and Ennis Railways, and was subsequently for two years and a half Manager-in-Chief of the Cork Tunnel-works of the Great Southern and Western Railway. These works he brought to a successful completion in 1857. In January 1858 he left England for India, under a covenant with the Bombay, Baroda, and Central India Railway Company, by whom he was engaged as an assistant engineer. In June 1860 he was promoted to the rank of Resident Engineer, and in the following month Acting Deputy-Chief-Engineer, in which appointment he was confirmed on the 1st of January, 1861. In May 1864 he was appointed Chief Resident Engineer of the entire Bombay, Baroda, and Central India Railway, and on the 14th of March, 1876, he became Agent of the line, still retaining his position of Chief Engineer. Mr. Mathew had previously officiated as Agent on two occasions, for six and eighteen months respectively. In 1866 he was a Justice of the Peace for the town of Bombay, and was a representative of the Justices on the Municipal Corporation. In 1874 he was appointed one of the Trustees of the Port of Bombay by the Government, and on the reconstruction of that Board was re-elected by the Chamber of Commerce. He was also elected Chairman of the Committee, and Deputy Chairman of the Chamber of Commerce, and in 1874 a Fellow of the University of Bombay. He was a member of the Committee of the Bombay Branch of the Royal Asiatic Society, Chairman of the Board of Directors of the Colaba Company, and President of the Sassoon and Parell Mechanics' Institutes. In 1879 he was chosen to succeed Colonel J. Pitt-Kennedy as Consulting Engineer to the Bombay, Baroda, and Central India Railway Company in England, but he did not take up the appointment till 1882. He continued to act in that capacity until his death on the 30th of September, 1885.

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[1] This memoir is mainly compiled from a notice in the *Times of India* for October 23, 1885.

Mr. Mathew was elected a Member of the Institution on the 7th of December, 1869.

WILLIAM RHODES was born on the 8th of April, 1820, at Fort Augustus, Invernesshire. His education was conducted at Bandvie and Inverness until he was fourteen, when he went to Edinburgh, and was at the High School there from 1836 to 1838. He was in the office of Mr. Joseph Mitchell, M. Inst. C.E., Inverness; from thence he went for five years to the establishment of Messrs. Fenton, Murray, and Jackson, Engineers, Leeds, and was engaged constructing and fitting up locomotives, stationary and marine engines, also superintending the erection of dredging-engines on the River Shannon, under Mr. Thomas Rhodes, M. Inst. C.E.; then, from 1843, for three years he was similarly engaged in the establishment of Messrs. Buddicom and

Co., of Rouen, Evreux, &c. From 1846 to 1849 he was employed on the Leeds and Dewsbury Railway as assistant engineer to Mr. Henry Renton, M. Inst. C.E., then for two years he was superintending (for the War Department) the erection of fortifications at Alderney. For upwards of twelve years he was employed by Messrs. Thomas Brassey and Co., for seven years of the time as agent on the Paris and Cherbourg Railway, and for four years on the Bilbao and Tudela Railway, and was for some time on the Central Argentine Railway, and since July 1869 had been Resident Engineer for the Commissioners of the Caledonian and Crinan Canals, and since January 1878, on the Caledonian Canal, till the time of his death, which took place quite suddenly from heart disease, at the official residence, Clachnaharry, Inverness, on the 21st of July, 1885.

Mr. Rhodes was conspicuous for his unimpeachable integrity and for exercising the full amount of his energies on whatever he was engaged upon, and was highly esteemed by those who knew him best. He was elected an Associate of the Institution on the 9th of January, 1866, and was transferred to the class of Members on the 31st of March, 1885.

JOHN GEORGE BLACKETT, eldest son of Mr. John Blackett, M. Inst. C.E., the present Engineer-in-Chief of New Zealand, was born at New Plymouth, in March 1852. Mr. Blackett was educated at Nelson College, where he made a very creditable appearance, having taken a Government Scholarship. He joined the New Zealand Public Works Department as a cadet in 1871, being articled to Mr. John Carruthers, M. Inst. C.E., then Engineer-in-Chief. During his cadetship he was chiefly engaged on the survey and construction of the railway over the Rimutaka Range.[1] This was an excellent school for a young engineer, and Mr. Blackett availed himself fully of the opportunity it afforded for getting a good knowledge of his profession.

After completing his apprenticeship, he was for three years engaged as assistant engineer on railway-works and surveys in Nelson, Wellington, and Otago. On the recommendation of Mr. William N. Blair, M. Inst. C.E., then Engineer-in-Charge of the Middle Island, Mr. Blackett was in 1878 appointed Resident Engineer for the Nelson and Marlborough Districts, which office he held till his death. In this capacity he had charge of the Nelson-

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[1] Minutes of Proceedings Inst. C.E. vol. lxiii. p. 50.

Roundell and the Picton-Awatere Railways, and extensive roadworks through a wide and rugged country. He constructed bridges over ten large rivers, and the main roads between Blenheim and Nelson, and Nelson and the West Coast, were completed during his tenure of office. He died at his home in Nelson, on the 13th of September, 1885, after a few days' illness, from a neglected cold, which culminated in inflammation of the lungs.

Mr. Blackett was skilled in his profession, a faithful public servant, a genial companion, and a true friend. The Parliament of New Zealand was in session at the time of his death, and the Hon. Edward Richardson, C.M.G., Minister for Public Works, referred to him in the following remarks, which are extracted from the "New Zealand Hansard."

"I wish to take this opportunity of expressing my very deep regret at the death of Mr. George Blackett, the son of the Engineer-in-Chief of the colony, one of the first cadets who joined the Public Works Department in 1871. Through his zeal and steady attention to his duties, he rose to the responsible position of Resident Engineer, and in that position he won the respect of every officer of the department. He was looked upon as a most promising officer by every person who has held the office of Minister for Public Works in the colony, and he won the good feeling and, I might almost say, affection of the people of the Nelson District, to which he was attached. Sir, this gentleman was a bright example to all the young men rising in the service of the Government, and by his premature death the colony has lost a most valuable officer." Mr. Levestam, Member for Nelson, and Mr. Samuel, Member for Taranaki, also spoke in the same strain.

Mr. Blackett was elected an Associate Member, on the 28th of May, 1878.

REDMOND JOHN BROUGH was the eldest son of the late Mr. Secker Brough, Judge of the County Court of the County of Huron, Ontario. After preliminary studies at the University of Toronto and elsewhere, he began the active practice of his profession in January 1870, as assistant to Mr. Thomas Weatherald, P.L.S., on the Ontario Government survey for the drainage of the Grey Swamp, in the county of Huron. This comprised all the preliminary engineering work necessary for the preparation of plans and estimates for the drainage of this tract of country. After the conclusion of the field work, he had the preparation of all the plans and sections, as well as other office work.

In June 1870 he received an appointment on the engineering staff of the Great Western Railway of Canada, under Mr. George Lowe Reid, M. Inst. C.E., the Chief-Engineer, and was engaged on the preliminary and location surveys of the Canada Air-Line Railway, until the autumn of the same year. In November he was appointed junior Assistant-Engineer on the work of construction of the western division of that road, under Mr. J. St. V. Caddy, Division Engineer. In the following year, 1871, he was advanced to the grade of First Assistant Engineer, which position he held up to the date of the completion of construction in July 1873. He had during that time experience of the whole construction, from the commencement of the earthwork of the road-bed, to the completion of the various structures upon it, as well as the station-buildings, yards, watering-stations, &c. While employed upon the Air-Line Railway, he was entrusted with an inspection of the Mooretown Branch of the Canada Southern Railway, and reported upon the progress of that work to the Chief Engineer of the Great Western Railway. In July 1873, the construction of the Air-Line having come to a conclusion, he was appointed to the charge of a section of the work of double-tracking the main line of the Great Western Railway, under Mr. H. C. Symmes, Division Engineer, in which position he continued until November of the same year, when the work was completed. At that date, upon the recommendation of Mr. John Kennedy, M. Inst. C.E., then Chief-Engineer of the Great Western Railway, he received an appointment on the Toronto Waterworks, as assistant to the Chief-Engineer, Mr. P. A. Peterson, M. Inst. C.E. From that time forward he served as Assistant-Engineer in charge of various portions of this work. In 1876 Mr. Peterson resigned the position of Chief-Engineer and Mr. Brough was appointed Resident-Engineer in charge of the construction of the works under the late Water Commission. Upon the transfer of the works from the Water Commission to the Corporation, in the beginning of 1878, he was appointed Permanent Engineer and Manager. In 1878, he succeeded the late Mr. Frank Shanly as City Engineer of Toronto, which position he held up to the time of his death. On the 12th of July, 1883, while inspecting a portion of the city works, he was thrown from his carriage, breaking his thigh and receiving serious internal injuries, which resulted in his death on the 21st of the same month, in the thirty-seventh year of his age.

Mr. Brough was noted for his strict conscientiousness in the discharge of duty, and for the uniform kindness and impartiality with which he treated all who were engaged in carrying on the works under his control. His abilities as an engineer were of a high order, and would have ensured him a brilliant future had not death put an end to a career in which he was already making rapid advancement. Personally he enjoyed the respect of all who knew him, and endeared himself to a circle of faithful friends, by whom his death is sincerely mourned.

He was elected an Associate Member of the Institution on the 2nd of December, 1879. He was a member of the American Society of Civil Engineers, and also a member of the American Society of Mechanical Engineers.

WILLIAM JACKSON began his career as an apprentice in the works of Messrs. Otway, Winnington and Co., mechanical engineers, where he remained for six years. On the completion of his apprenticeship in 1852 he began a series of short engagements with various firms of mechanical engineers, seldom remaining more than a year or two in one employ, until, in 1858, he was appointed by the "West India Royal Steam-Packet Company" as Engineer afloat, on board s.s. "Conway," s.s. "Tasmania," s.s. "Oneida," s.s. "Douro," and Chief Engineer of s.s. "Clyde" and s.s. "Trent." He retained this position until 1865, when he was engaged by Messrs. John Penn and Son, marine engineers, as Engineer afloat, in the service of the Khedive of Egypt, by whom he was made Chief Engineer of H.H.'s yacht "Fayoum" for two years. He was afterwards for a year in Messrs. Penn's works at Greenwich. In 1868 he undertook the situation of mechanical engineer at the extensive Cannock Chase Collieries, and this post he resigned a few months only before his death.

Conscientious, in some respects almost to a fault in the matter of duty, he was an exceptionally hard worker, and the exigencies of collieries requiring new work and important repairs to be carried out week-ends and holidays, he was, during the last sixteen years of his life, incessantly engaged. It is characteristic of the man that for a long period of years he never slept away from his house adjoining the works, and when suffering from fatal illness, partially paralysed and unable to move without great pain, he would insist on visiting the shops when any temporary return of strength enabled him to move without assistance. His attention to detail was equal to his capacity for work, and he brought to bear on colliery mechanics much of the nice and correct workmanship of large engineering works. To a great extent this close and almost microscopic attention to detail interfered with original work, but in underground haulage and other work connected with colliery engineering he displayed very considerable mechanical skill, his extreme nicety in studying detail often approaching originality, so much did he analyze conditions which in an ordinary way appeared unnecessary. With the same amount of time spent over a more ambitious field of invention he would probably have been a prominent man. It may be said that he sacrificed himself to a too close attention to minor details, and instead of subverting such knowledge, at a time when it was fairly perfected, to a more general sphere of usefulness, the habit became ultimately predominant. His sensitiveness of character, aggravated by disease of long standing, also contributed to his remaining in a position which, although an onerous and responsible one, did not afford him an opportunity of individualizing himself.

Mr. Jackson was elected an Associate Member on the 1st of March, 1881. He died on the 22nd of March, 1885.

SAMUEL ROBERT LINGING, son of Mr. Edward Meek Linging, was born in 1851. On the completion of his education, in 1871, he was sent to the Hull works of the British Gaslight Company, where he was appointed assistant to the then Engineer and Manager. Four years later he was made Engineer and Manager of the Potteries Station of the same company, a position he held until his untimely death on the 5th of October, 1885. His career was too short to allow of his name coming prominently before the gas-world, but his services were such as induced the Directors of the company to place the utmost confidence in his skill as an engineer, and in his general business qualifications, and on his death they unanimously passed the following resolution :—

"The Directors receive with the deepest regret the intelligence of the death of Mr. Robert Linging, their Manager at the Potteries Station. They desire to record their sense of the most valuable service rendered by him to the Company during the ten years of his management. His constant energy and skill were of the utmost value to the Company, and he combined with these qualifications so much kindness and courtesy, that he gained the regard and esteem of all who were brought in contact with him."

Mr. Linging was elected an Associate Member of this Institution on the 2nd of April, 1878.

JAMES HENRY WALLER, second son of the late Mr. James N. Waller, J.P., D.L., of Allenstown, Co. Meath, Ireland, was born at Allenstown, on the 25th of March, 1845; and died in Banda Oriental, South America, on the 13th of March, 1884. He began his engineering career under the late Mr. William Martley, M. Inst. C.E., and having completed his apprenticeship was for three years employed in the locomotive-department of the London, Chatham and Dover Railway. He went to India in 1868 on a three years' agreement as Locomotive-Superintendent of the Sind Railway, on leaving which in 1871 he was presented with an address and valuable gold watch by the European and Native employées as a mark of their esteem and respect. Having spent a short time at home to recruit his health, which had been much tried by his residence in India, he proceeded to Iquique in South America, and was employed on railway work there until 1874, when he joined a brother in Banda Oriental, bought land, and took to sheep-farming, in which occupation he was successfully engaged up to the time of his death, which took place suddenly. Mr. Waller was elected an Associate Member on the 3rd of December, 1872, having previously been a Student.

MICHAEL PATTERSON, born on the 22nd of March, 1830, was the elder son of Mr. Thomas Patterson, who for about forty years occupied the position of Engineer at the Shotley Grove Paper-Mills of Messrs. Annandale and Son, where Michael Patterson served his apprenticeship as fitter and millwright. During this time he occupied every spare moment in study. He visited the Great Exhibition of 1851 with his father, and while there succeeded in getting employment with the late Mr. Marshall, of Dandy-Roll fame. Mr. Patterson's energy and perseverance were so much appreciated that he was entrusted with the erection of a large drying-machine for Messrs. Allen and Sons, Ivybridge. This step led to his being appointed, at the age of twenty-three, Engineer of the celebrated Ivybridge Mills, which subsequently, under his direction and skill, became one of the finest mills in the British paper-trade. Messrs. Allen and Sons permitted Mr. Patterson to act as consulting engineer on his own behalf. They, moreover, appointed him Engineer for the Old Delabole Slate-Quarries, and during his residence in the West he was largely consulted on engineering matters of importance. From Ivybridge Mr. Patterson went, in 1874, to Edinburgh, having accepted an engagement as Manager of the St. Katharine's Engineering Works of Messrs. George and William Bertram. He there became very closely associated with the late Mr. George Bertram, and his great reliability, urbanity and genial manners made for him a large circle of friends throughout the paper-trade. In 1884 Messrs. Bertram resolved to found the new St. Katharine's Works, Bow, London; and the management was undertaken solely by Mr. Patterson. At the time of his decease he was there vested with complete authority as Manager, quite independent of the control of the Edinburgh firm.

Mr. Patterson died in London on the 16th of December, 1885, in his fifty-sixth year. His remains were removed for interment to Edinburgh, where nearly two hundred members of the paper-trade testified their respect by being present at the funeral. He was elected an Associate of the Institution of Civil Engineers on the 4th of December, 1866.

⁂ The following deaths occurred in the three months ending on the 31st of December, 1885, in addition to those included in the foregoing notices:—

Members.

HEMANS, GEORGE WILLOUGHBY, on the 29th of December.
SWANWICK, FREDERICK, on the 15th of November.
WILSON, GEORGE (of Sheffield), on the 1st of December.

Associate Member.

TURNER, ALBERT HARRISON, on the 5th of December.

Associates.

BAGNALL, THOMAS, on the 11th of October.
CROSSLEY, EDWIN, on the 15th of October.
HARTLEY, FREDERICK WILLIAM, on the 17th of October.

Information respecting the career and leading characteristics of any of the above is solicited, to aid in the preparation of future Obituary Notices.—SEC. INST. C.E.

SECT. III.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS.

Experimental Investigations on the Laws of Compressive Strength.

By J. BAUSCHINGER.

(Mittheilungen aus dem Mechanisch-technischen Laboratorium der k. polytechnischen Schule in München, 1876, p. 1.)

The general theory of the strength and elasticity of materials originated among others by Cauchy, Poisson, and Lamé, and developed by Saint-Venant and Clebsch, gives scarcely any information as to the connection between the crushing-strength of a body and its form.

Experiments to ascertain this connection were made by Rondelet, Vicat, and Hodgkinson, of the results of which the Author gives a summary, and then describes his own investigations in this direction, made with cubes, prisms, and cylinders of sandstone and of cement.

The Author finds that the formula—

$$\beta = \sqrt{\frac{\sqrt{F}}{\frac{u}{4}}\left(\lambda + \nu\,\frac{\sqrt{F}}{h}\right)}$$

fairly represents the results of these experiments, where β_0 is the crushing-stress in kilograms per square centimetre, F the area and u the circumference of the section, h the height of the prism or cylinder, and λ and ν experimental constants depending on the nature of the materials. This formula also agrees well with experimental results previously obtained; according to it, prisms (including cylinders) of geometrically similar form when made of the same material have equal crushing-stresses; further, for prism of equal height and equal sectional area the crushing-strength is proportional to the square root of the circumference of the cross section.

In six Tables the Author gives the particulars of the experiments on which the above formula is based and a comparison of the observed and calculated results.

The chief forms of the fragments produced by crushing stone of all kinds are without exception the wedge and the pyramid (for cylinders, the cone), having their bases in the end surfaces of the test-piece to which the pressure is applied, and the slabs broken off from the sides.

With prisms or cylinders of a height equal to the side of the square circumscribing the base, two pyramids or cones are formed having their bases in the surfaces under pressure; these are frequently of equal height, but more often very different; their height decreases as that of the test-piece becomes less than the height of the side of the circumscribed square, and this also applies to the area of the base, which for a cube equals the whole of the surface under pressure. When the height of the test-piece exceeds that of the cube, the fragments maintain the pyramidal (or conical) form up to a height of about 1½ time the greatest diameter; beyond this, however, pyramids and cones are gradually replaced by wedges, which invariably appear at a height equal 2½ times the greatest diameter, and the length of which increases with that of the test-piece, while the strength of the latter gradually decreases.

The Author's formula applies to prisms or cylinders until the height equals four or five times the greatest diameter of the base.

Two further series of experiments were made, in one of which the pressure was applied to a portion only of the end-surfaces of the prismatic test-pieces, and uniformly distributed, while in the other the distribution was not uniform. For the former experiments small steel cubes were used for concentrating the pressure on a portion only of the surfaces of the test-pieces. When one surface was entirely supported, while pressure was applied to a part only of the other, fracture resulted in a pyramid having the smaller surface under pressure for its base being driven into the test-piece.

When two steel tubes of equal dimensions were used at either end for applying pressure to a cubical test-piece, the results showed that the force required for crushing was the same as if the test-piece were a prism with a base equal only to that of the steel cubes and of the height of the test-cube, the material surrounding this prism appearing not to affect the strength.

In the case of the distribution of pressure not being uniform it was found that rupture began at the most critical points when the crushing-stress there attained the intensity at which rupture would take place with a uniformly distributed load.

The results of the two latter series of experiments are, like the previous ones, given in a tabular form, and the Paper is accompanied by fac-similes of photographs taken from the test-pieces after rupture.

G. R. B.

An Instrument for recording simultaneously the Vertical and Horizontal Oscillations of a Girder. By Dr. Pio Chicchi.

(L'Ingegneria Civile e le Arti Industriali, 1885, p. 119.)

Two cylinders, one turning on a vertical, the other on a horizontal axis, are connected by bevelled wheels so as to move simultaneously, and in order to avoid a complicated system of clockwork, the Author designed a simple method of obtaining practically continuous motion. A piston, working in a cylinder, is connected by a cord passing over pulleys, and round a spindle upon the axis of one of the revolving cylinders, with a weight. The cylinder in which the piston moves is filled with water, and a pipe outside it is connected with it so as to form a communication (which can be opened or closed by means of a tap) between the water above and that below the piston. When the tap is closed the piston remains stationary. If the piston is at the bottom of the cylinder, and the tap is opened, the weight will cause the piston to rise, and the rate of motion can be regulated by opening the tap more or less.

The apparatus is placed upon a light staging (a single pile being sufficient in many cases) underneath the girder, and two pencils suitably connected with it are arranged in contact with the two revolving cylinders. When a load is ready to pass over the bridge, the tap is opened and the instrument set in motion, and as the load passes over the bridge the pencils record the oscillations. If no permanent set takes place, the pencils will, after the load has passed, return to the line they first marked, but if there has been a permanent set this will not be the case.

Drawings, and a detailed description are given in the Paper.

W. H. T.

The Preservation of Timber.

(Transactions of the American Society of Civil Engineers, July 1885, p. 247.)

The report of the Committee of the American Society of Civil Engineers on the preservation of timber was presented in June 1885. The Committee, consisting of Mr. O. Chanute and others, was appointed early in 1880, and made a preliminary report in 1882. The present report is the outcome of long and laborious research, directed to the determination of the most successful methods of preserving timber from decay—best adapted to the needs and current practice in America; as it appeared that several of the sources of supply were being rapidly exhausted, and that it would soon become imperative to resort to artificial preservation of wood, as had been done successfully for many years in Europe.[1]

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[1] Minutes of Proceedings Inst. C.E., vol. lxxviii., p. 97.

The various experiments made in the United States, on which information was collected, are classed as follows:—

1. Kyanizing, or use of corrosive sublimate (chloride of mercury).
2. Burnettizing, or use of chloride of zinc.
3. Creosoting, or use of creosote oil.
4. Boucherie's Process, or use of sulphate of copper.
5. Miscellaneous.

From the results of various experiments, some of which lasted many years, with kyanized timber used for railway-sleepers, fortifications, bridges, and floors, it is deduced that for bridges, trestles, fences, and like exposed structures, kyanizing is a useful process, and may safely be relied upon to preserve the wood for from twenty to thirty years; but that for railway-sleepers, pavements, and other works exposed to constant moisture, success is doubtful, probably in consequence of the washing out of the corrosive sublimate.

The floor timbers of an engine-house at Charlestown were kyanized "because the location was very damp," and they were found, a few years afterwards, as much decayed as if they had not been so prepared.

The Committee consider Burnettizing less adapted to bridge-work than to the preservation of railway-sleepers. In the Havre de Grace bridge, pine timber, Burnettized, proved to be brittle. When zinc-solutions are employed, weak enough not to impair the strength of the timber, they are likely to be washed out by the action of rain and moisture, and to leave the timber unprotected. For instance, the sleepers on the Chicago, Rock Island, and Pacific Railroad were externally decayed and exfoliated. Burnettizing, thoroughly done, is recommended for the preservation of railway-sleepers.

Preservation by creosoting is the standard method of treating timber in the United Kingdom. Of twelve leading railway companies in England who were addressed in 1878, ten companies used creosoted sleepers, some of which, forwarded as samples, were still sound, after twenty-two years of exposure. As a protection against marine worms, creosote is thoroughly efficient. Although, in English harbours, a charge of from 10 lbs. to 12 lbs. of creosote per cubic foot of timber has proved sufficient, it appears that for the higher temperatures of sea-water on the French coast, and in the southern harbours in the States, a charge of about 19 lbs. per cubic foot is required to secure immunity from attack. Though creosote is cheap and abundant in England, it is comparatively scarce and dear in America. The price of labour there, also, is higher, and longer time is required to operate upon freshly-cut timber, insomuch that in most cases it was found by calculation that it cost less to let the timber rot, and to replace it, than to incur the expense of preparing it against decay. "Nothing is more curious," say the Committee, "than the way in which all the inventors of cheap processes for preserving wood quote the ancient Egyptians."

Crude petroleum, by excluding moisture in the case of railway sleepers, has proved to be preservative so long as the wood continued to be saturated with it, as on some of the sidings of the Oil Creek Railroad, used for loading oil; but, if merely injected once for all, it evaporates out of the wood, leaving it unprotected.

Dr. Boucherie's process, forcing a solution longitudinally through the timber, has been extensively applied in France for many years, with satisfactory results. But, to be successful, it must be applied to freshly-cut trees in the log only; and this treatment involves so much delay, moving about, waste, and annoyance, as to lead to its abandonment. These difficulties would be still greater in America, and in the Northern States the process could not be applied at all during the winter—the season for cutting down trees—as the solution would freeze. Mr. H. Fladd, of St. Louis, introduced, in 1882, a method which is the inverse of the Boucherie process. To the cap fastened to the end of a freshly cut log he applies a suction-pump, and, placing the other end into a vat, filled with the desired solution, he sucks up the preserving fluid through the pores or sap-cells of the wood. Many railway sleepers have been treated in this way with various chemical solutions, the chief of which was sulphate of copper; and it is probable that the life of the wood is thereby increased.

Several experiments have been made with other substances. The Earle process, by which timber is immersed in a hot solution of sulphate of copper and sulphate of iron, was tried at the Watervliet Arsenal. The life of the wood was lengthened, but the strength was impaired, and the wood warped so badly that the process was abandoned in 1844. The lime process, consisting of soaking timber in lime-water, was applied in 1840, by Mr. W. R. Huffnagle, to white-pine sills laid in the permanent way of the Columbia Railroad, and in 1850 on the Baltimore and Ohio Railroad. The results were not satisfactory. Salt has been experimented with numberless times, with but limited success. The charring of timber is uncertain, and disappointing in its results. In 1865, Mr. B. S. Foreman introduced the application of a dry powder for preserving wood, composed of salt, arsenic, and corrosive sublimate. It proved to be fatal to men and animals. Railway sleepers so prepared were licked by cattle for the sake of the salt. The cattle died, and the line was strewed with dead cattle for ten miles. The farmers rose in arms, and forced the company to take up and burn the sleepers. Many other processes are noticed by the reporters.

The Report comprises much tabular matter; and it concludes with a consideration of the decay of timber, the selection of preserving-processes, the conditions of success, and the question, Will it pay? There are, in addition, twenty appendices on the various processes, contributed by various authorities. In these appendices are given three different methods of estimating the economy to be derived by using some process for preserving ties. Mr. Andrews gives the money-saving at the end of the term of life of the preserved tie; Mr. Harrod gives the life that the preserved tie must have in order to make the cost of using preserved or unpreserved ties equal; and Mr. Welch gives the money-value of any tie in use when compared with the white-oak unpreserved tie taken as a standard; the difference between the money-value and the cost showing the economy or the loss in use. In the first and last of these investigations, as Mr. F. Collingwood points out, the Authors have compounded the interest on the several costs; in the second case, simple interest only has been taken. This is a point that must be settled before any close agreement can be reached, and Mr. Collingwood contributes some elaborate calculations with Tables, towards a solution.

In the course of the discussion which followed the reading of the Paper, particular attention was given to the comparative cost of iron sleepers and wood sleepers, including interest on outlay.

Mr. Collingwood contributes an elaborate appendix on "The Preservation of Forests." From a consideration of statistical data, it is estimated that the supply of first-growth white pine will be practically exhausted in the course of eleven years. The lumber cut in a single year from the district of Michigan, Wisconsin, and Minnesota, would load a train of cars nearly 7,000 miles in length. Vast quantities of hemlock in the north have been destroyed for nothing but the bark; and in newly-settled regions great quantities of wood of various kinds are burned for the purpose of clearing the land. Years ago, the elms in the province of Quebec were burned for the manufacture of potash from the ashes. But the greatest enemy is a forest-fire. It is estimated that, in Ottawa, ten times as much timber is burned as is cut. The great unknown factor, Mr. Collingwood concludes, is waste. If this be done away with, and reasonable care be taken to re-plant forests, an abundance of timber may be secured for generations to come.

D. K. C.

Record of Tests of Cement made for the Boston Main-Drainage Works, 1878-1884. By E. C. Clarke.

(Transactions of the American Society of Civil Engineers, vol. xiv., 1885, p. 142.)

The experiments described in this Paper were made with reference to the supply of cement for the drainage works of the city of Boston, Mass. The consumption of cement in these works was about 180,000 barrels. Several kinds of natural and artificial cements were examined, and numerous experiments made with them. Drawings of the apparatus used are given.

The breaking-section of the briquettes used for testing the tensile strength was 1 square inch at first, but a larger section of 2¼ square inches was ultimately adopted. The shape of the briquettes resembled that in use in this country. The colour of the cement was found to be of little use as an indication of quality, except in the case of some Portland cements, the yellowish hue of which showed them to be under-burned.

For ascertaining the weight per cubic foot of the cement, a box containing $\frac{1}{10}$ of a cubic foot was fixed 3 feet below a coarse sieve, with which it was connected by means of an iron tube. On the cement being shaken through the sieve it fell into the box, with the top of which it was then struck off level. The weights per cubic foot of different cements when determined in this way varied considerably. Rosendale cement weighed 49 to 56 lbs. per cubic foot, while an American Portland weighed 95 lbs. The more coarsely a cement was ground, the greater was its weight per cubic foot. No direct ratio could be discovered between weight and strength; but, as a general rule, heavy cement, if thoroughly burned and finely ground, was preferred to light cement. Great importance was attached to the test of fineness, standard sieves varying from No. 50 to No. 120 being used. As the actual number of meshes was sometimes as much as 10 per cent. less than the trade number, it was customary in the case of important contracts to compare two sieves, one of which was retained, and the other given to the manufacturer for his guidance. The No. 50 sieve was found to be too coarse, some cements leaving no residue upon it, while they appeared quite coarse when tested by the 120 sieve. This latter sieve was found to be about as fine a one as it was practicable to use, on account of the time required to sift the cement through it. It was therefore adopted as a standard. The difference in price between a cement of which 70 per cent. passed the 120 sieve, and one of which 88 per cent. passed the same sieve, was 88 cents (3*s.* 8*d.*) per barrel, and it was found to be much more economical to use the latter.

Tests were made both with neat cement and sand mixtures, the latter being preferred. The strength of briquettes made of neat cement did not always indicate the capacity of these cements to bind sand. The usual proportion of sand was three parts to one of cement, the kind used being rather coarse, clean, sea-beach sand. The proportion of water used in gauging the cement varied with the different brands, and was so chosen that the mortar was somewhat stiffer than that commonly used by masons. The moulds were of brass, and were placed upon a marble slab for filling. The materials for one briquette at a time were weighed out and carefully mixed, the mortar being rammed into the mould with a wooden rammer, and its surface afterwards smoothed with a trowel. The time of setting of the cements was determined by noting the period which elapsed before the cement would bear a wire $\frac{1}{12}$ inch diameter loaded with a $\frac{1}{4}$ lb. weight, or a wire $\frac{1}{24}$ inch diameter loaded with a 1 lb. weight. In the latter case, the setting of the cement was assumed to be complete. No direct relation was established between initial energy and subsequent strength. By mixing quick- and slow-setting cements, a mixture could be obtained which would set within any desired period.

The value of an untried brand of cement could hardly be ascertained in less than a month. In one case, briquettes made with a new kind of cement showed a strength of 267 lbs. per square inch in one month, but fell to pieces in six months.

The testing machine for breaking the briquettes was worked by levers acting on a spring balance, which was tested from time to time, and found to maintain its accuracy.

A number of experiments, the results of which are given in detail, were made to elucidate special questions. Portland cement was found to acquire its strength more quickly than Rosendale. In a series of tests extending over two years, it was found that when ten or more parts of sand were mixed with one part of Portland cement, the strength at the end of the second year was less than at six months. With Rosendale cement this diminution took place with mortars composed of four or more parts of sand to one of cement. Twenty-four hours after mixing, Rosendale has about three-quarters the strength of Portland cement. After the lapse of a week, however, the Portland cement is three times as strong as the Rosendale, but in two months it is only twice as strong.

In order to ascertain the influence exerted by the size of grain of the sand used for testing, a special series of tests were made. These show that it is advisable to employ sifted sand of nearly uniform size. Coarse sand makes a stronger mortar than fine, but sand containing coarse and fine particles mixed makes nearly as strong a mortar as the coarse. An experiment made to ascertain whether a breaking-section of 1 square inch or 2¼ inches was preferable, showed that neither size, as a rule, gave higher results than the other.

Comparative tests were made with briquettes which were moulded upon porous or non-porous surfaces. For the first one or two months the porous bed gave slightly higher results, but the difference disappeared with age. To diminish the personal influence of the operator in filling the moulds, a machine was devised for carrying out this operation. Its action was satisfactory, but its use was abandoned on account of the greater time required to make briquettes with it.

The influence of salt water mixed with cement mortar was the subject of a series of experiments. Although salt water was found to retard the setting of cement, it had no important effect upon the strength.

A great number of tests were made to ascertain the most favourable proportion of water in mixing cement. It was found that 20 to 25 per cent. of water gave the best results with Portland cement, and 30 to 35 per cent. with Rosendale cement. Although the differences in strength due to the amount of water are considerable at first, they diminish greatly with age.

Finely ground cements were found to be weaker than coarsely ground when tested neat, but with an admixture of sand the positions were reversed. In one instance a finely ground cement had the same strength neat as a coarsely ground one; but with four parts of sand the strength of the former was double that of the latter. German cements were found to be more finely ground than English. It was impossible, by tests on the tensile strength of neat cements alone, to judge of their value in making mortar for practical use. By removing the coarser particles of a cement by means of sieves, it was found that the efficiency of a cement for making mortars with sand increased in proportion as the coarser particles were removed. Rosendale cement was not affected to the same extent as Portland by differences in grinding. Experiments were made to ascertain whether the coarse particles of cement remaining on a No. 120 sieve had any cementitious value. They produced mortar of greater strength than when sand was substituted for them.

For wet work it was found advantageous to mix Rosendale and Portland cements in equal proportions, the resulting mortar being but little inferior to that made with Portland cement and sand.

To ascertain the influence of loam on mortar, some experiments were made with briquettes, the sand in which contained 10 per cent. of loam. At one month the breaking loads were only about one-half what they would have been had clean sand been used, but at six months and one year little difference was observable. For stopping leaky joints, a mixture of clay with cement-mortar was used, and a series of tests made with briquettes composed of this mixture indicated that the presence of clay in moderate amounts does not weaken cement-mortars, nor were such mortars found to suffer from an exposure to the weather during a period of two and a half years. Another mixture for stopping leaks through joints in sewers was made of melted tallow mixed with Portland cement and sand, equal parts. In one week this compound acquired a tensile strength of 40 lbs. per square inch, and this did not increase in a year.

To test the transverse stress which concrete structures will sustain concrete beams were made 10 inches square, and about 6 feet long. These were broken after having been buried in the earth for six months. Concrete, especially when made with Rosendale cement, was found to have a rather low modulus, and it is recommended to give concrete ample time to harden when transverse stress is to be opposed. Experiments were made to ascertain the resistance which cement-mortars offered to abrasion. Blocks were moulded 1½ inch square, and allowed to harden eight months. They were then pressed against a grindstone, with a pressure of 20 lbs. The number of revolutions of the grindstone required to grind off 0·1 inch of each block was noted. Cements opposed the greatest resistance to abrasion when combined with the largest proportion of sand which they could bind, without allowing the grains to be pulled out in grinding.

Cements gauged neat, and with different admixtures of sand, were filled into lamp-glasses, in which they were allowed to set. The glasses were immersed in water with their contents, and in three days all of them began to crack. An attempt was made to measure the expansion of 10-inch cubes, the greatest lineal expansion being 0·001.

Cement-mortars placed in boxes made of different kinds of wood were found not to be acted on in any way at the end of a year. A mortar composed of one part of Portland cement and two parts sand was allowed to harden for a week, when it was pulverized, and again made into briquettes. After the lapse of two years, these briquettes attained a strength of 93 lbs. per square inch.

Samples of a selenitic cement were tested in comparison with the same kind of cement before it had been treated by the selenitic process. The treatment to which it had been subjected had not improved the cement sufficiently to compensate for the increased cost. W. F.

The Tallest Chimney in the World.

(Dingler's Polytechnische Journal, vol. cclviii., 1885, p. 140.)

In the summer of 1884, a new chimney-stack was commenced by the Mechernich Lead-Mining Company, which had reached a height of 23 metres when the works were discontinued on account of the autumn storms commencing. On the 14th of April, 1885, the building was resumed, and on the 19th of September following the total height of 134·6 metres was reached. The leading dimensions are as follows: The foundation, in dressed stone masonry, is 11 metres square and 3·5 metres high. The base, a cube of 10 metres, and the octagonal plinth of the shaft, are built of annular-kiln bricks. The shaft, of circular form, in radial bricks is 121·1 metres high, 7·5 metres outside, and 3·5 metres inside diameter at the base, and 3·5 metres outside and 3 metres inside diameter at the top. The height, of 134·6 metres (441·6 feet), is 2·1 metres (6·9 feet) more than that of Tennant's chimney, at St. Rollox, which is given as 132·5 metres (434·7 feet).

H. B.

Oscillation of Chimneys. By E. Bourry.

(Mémoires de la Société des Ingénieurs Civils, June 1885, p. 721.)

The amplitude of the oscillation of chimneys has been exactly measured by observation of the shadows cast by the sun upon the ground. Recently, the oscillations of a chimney 115 feet high and 4 feet in diameter externally at the top, near Marseilles were observed, by the shadow, during a high wind, to attain a maximum of 20 inches. It was estimated that the chimney deflected by an initial impulse, would have made four or five oscillations before returning to a state of rest. On the contrary, by a succession of impulses isochronous with the oscillations, a chimney may finally be overthrown. Such is the explanation of the destruction of certain chimneys in which nevertheless all the conditions of statical stability were fulfilled.

D. K. C.

A New System for Subaqueous Foundations. By E. GAERTNER.

(Wochenschrift des österreichischen Ingenieur- und Architekten Vereins, 1885, pp. 19, 29.)

In carrying out pneumatic foundations an iron caisson of the form of the object required is generally used as a working chamber. The masonry carried up upon this is usually constructed within an iron sheathing, which serves to protect the new work from injury by friction as it is pressed down into the ground, and also acts as a cofferdam. The caisson and the sheathing remain permanently in the structure. Many attempts have been made to lessen the cost of this system, caused by the loss of the iron. Works have been carried out in which the caisson served only as a diving-bell, and was afterwards removed; but this method, apart from its great cost, only answers where the foundation does not go deep into the bottom, and then only for special cases.

In another direction saving of cost has been sought by removing the upper iron sheath after the masonry has been carried up sufficiently high, and using it again.

Already in 1851 Pfannmüller had suggested in a scheme for bridging the Rhine at Mainz that the upper portion of the iron sheaths for the piers should be screwed off when the masonry was carried up above the water-level, and used again at another pier. In the case of the Saltash Bridge, Mr. Brunel removed the iron cylinders, 37 feet in diameter, for the whole depth of about 56 feet, in which the piers were not imbedded in the bed of the estuary. In constructing the Antwerp quays, the contractors built the masonry, which was founded upon pneumatically sunk caissons, within massive iron sheathings, which, when the masonry was far enough advanced, were unbolted from the caissons, lifted bodily, and re-used at another part of the wall. As the foundation of the masonry did not go far into the solid, the friction to be overcome in lifting these sheathings was not great.

In 1882 the Società Italiana began the founding of a quay-wall, about 220 yards in length, along the corrected course of the Tiber for the protection of the Villa Farnesina. The work was all done in the dry, the great curve of the Tiber not being cut out till later. The wall was founded upon a series of iron caissons, 65 feet 7 inches long, 15 feet 9 inches wide, and 29 feet 6 inches deep. The masonry was brought up for 23 feet in rough tufa and puzzuolana-mortar, and for the remaining 6 feet with a facing of travertin blocks.

A sheathing of unusual construction, which was put up to its full height at once, was used, perhaps, rather as a protection during the sinking for the rough masonry in slow-setting mortar, than as a cofferdam in which to build the upper masonry. This sheathing consisted of vertical iron plates 23·6 inches wide, connected together by two flat bars riveted together, between which the edge of the plates was pushed. There was no filling or caulking of the joints, and no attachment to the caisson. When a length of walling was finished, the plates and bars were pulled out and used again in another length.

Inspired by these works the Author designed a "foundation mantle," which easily takes to pieces, and is adaptable to any form of caisson. It consists of vertical plates, 0·2 inch thick and 2 feet 7 inches wide, in 6 feet 7 inches lengths, joined together at the horizontal points by double coverplates and a double row of bolts. Laterally these plates are kept in position by fitting into a riveted I formed of two bars, 5·9 inches × 0·47 inch, separated by a bar, 2 inches × 0·27 inch, all three being riveted together. The vertical plates are not fastened to these I irons, but the joint is caulked. The I's break joint with the plates, and are only lightly attached to the caisson.

In the winter of 1883–4 the two abutments and two piers of the bridge over the Wisloca at Dembica, in Galicia, were erected upon this plan. The area of the foundation of the abutments is 68 square yards each, and of the piers 57 square yards. The foundations varied in depth from 20 to 26 feet below low water, and from 28 to 37 feet below the ground level, and four or five rows of the movable plates were used according to circumstances. The materials sunk through were sand and gravel first, and lower, stiff clay. When the foundation of one pier was completed and the masonry brought up high enough, the sheath was removed to the next pier. It was used four times.

To obviate any danger of bolt-heads or other projecting pieces catching on the masonry as it is being drawn out, the space between the sheath and the masonry is filled with sand as the latter progresses. The sheath is removed by windlasses, which draw out the vertical plates and fish-joints one by one. The joint between the sheath and the caisson is simply broken in this operation.

The weight of the sheath, including all joints and fastenings, is for the lowest row of plates 19 lbs. per square foot, and for the rest 15·6 lbs. If the plates have to be re-used over a caisson of different shape from the one from which they are being taken, it may be necessary to bend them to the required curve, and to this end they must be of the best material; the vertical fish-joints are the same for all shapes of caisson.

It had been intended to make careful tests to ascertain the amount of friction encountered in drawing off the sheath, but the floods of June 1884 made it necessary to get the work done as soon as possible, and the tests were therefore confined to the right pier, which was 115 feet from the bank, and founded 21·6 feet below low water and 38 feet below the surface of the ground, made up of—

Sand and clay	10·50 feet
Loose stones and sand	18·60 ,,
Firm clay	8·90 ,,

While the sinking of one of the caissons was in progress, the giving way of the joints of two of the vertical plates at their joining with the caisson, gave the opportunity of calculating the external friction, the bottom of the caisson being at the time 5·2 feet below low water, and 23·3 feet below the surface of the ground. This gave a resistance due to friction of 5·1 tons per foot run of circumference, equivalent to 492 pounds per square foot of surface of the sheathing.

These and some observations of other plates which gave way on the further sinking of the caisson, taking into account the degree in which they followed the caisson or stayed behind, showed that the earth-pressure on the inside of the sheathing is proportional to the friction just as is the case on the outside.

These slight observations showed that the coefficient of friction inside is at least as great, if not greater, than that outside, and that therefore passive earth-pressure produces as great an amount of friction as active earth-pressure. It was now of interest to determine the resistance to friction when both surfaces of the sheathing were sliding, as is the case when the sheathing is drawn off, instead of only one as in the former cases, and whether, as must be theoretically assumed, the resistances work simultaneously on both sides of the plate, and are thus proportional to the sum of the inner and outer coefficients of friction. If this is so a comparison of the theoretical calculated outside friction with the observed resistance to the drawing up of the sheathing should show the former to be at most one half the latter.

The calculations have been made for all the cases in which the plates were above water (there being no theory giving useful results for material permeated by water).

The theoretical earth-pressure was determined by Rebhann's Construction, in which the weight of the material was taken at 98·3 pounds per cubic foot, and the angle of repose for sand with loam at 38°, and gravel and sand 36°. After a long series of observations the coefficient of friction for the outside was found to be 0·466, and for the inside 0·588, or 20 per cent. greater than the outside friction. The results of the calculations were reduced to diagrams which thoroughly proved that with the simultaneous action of two surfaces of the same body (as in the case of the sheathing when being drawn off), the resistances to friction act simultaneously on the two surfaces, and therefore must be added together.

a.	*b.*	*c.*	*d.*
Depth below Surface of ground in Metres.	Mean Resistance to the Winding up of the Vertical Plates of the Sheathing.		
	Per Plate 2 feet 7 inches wide.	Per 1 Metre of Circumference of Sheathing.	Per Square Metre of Sheathing.
	Tonnes.	Tonnes.	Kilograms.
1·0 (3·28 feet)	0·140 (0·138 ton)	0·177 (0·053 ton per foot)	177 (36·3 lbs. per sq. foot)
2·0	0·560	0·709	354
3·0	1·390	1·759	586
4·0	2·620	3·316	829
5·0	4·560	5·772	1,154
6·0	5·550	7·025	1,171
6·5	6·290	7·962	1,224
6·75	7·290	9·227	1,367
7·10	9·150	11·582	1,631
7·50 (24·6 feet)	11·310 (11·13 tons)	14·316 (4·295 tons per ft.)	1,908 (391·1 lbs. per sq. ft.)

The above Table refers to the land pier on the right bank of the Wisloca. From this Table it is seen that the friction at considerable depths, and especially where the earth is saturated with water, increases very rapidly. The mean water-level in the case of the pier to which the Table refers was about 4·7 metres (15·4 feet) below the surface of the ground.

The observations and calculations described in the Paper were made by Mr. Adolf Titze, who was the engineer in charge of the works on behalf of the firm who built the bridge, the Author of the Paper being a member of the firm.

The Paper is illustrated.

W. B. W.

The Construction and Testing of Air-locks and Shaft-tubes for Sinking Foundations under Air-Pressure. By L. Brennecke.

(Zeitschrift für Bauwesen, 1885, p. 237.)

Although the question of the best method of design and of testing steam-boilers is one which has been generally carefully considered, and receives universal attention, a like investigation of a kindred subject, viz., that of the apparatus used for sinking foundations under a high air-pressure, has nowhere hitherto been carried out, although the consequences of an accident are likely to be even more disastrous in the latter instance than in the former, as in this case the men are actually enclosed in the apparatus, and so situated that they must in the event of an explosion be subject to its full effects. Also the strains which an air-lock sustains are of a very unfavourable character, owing to the vibration caused by the work proceeding in its interior, and the constant changes of temperature and of pressure to which it must be subjected when in use. The bursting of an air-lock is instanced, where the fractured portions on examination showed a highly crystalline structure, although the iron used in its manufacture had been of the best quality. Up to the present time the number of accidents known to have occurred in consequence of the bursting of air-locks is three, viz.: first, in 1865 at Zeche Rheinpreussen, where two men were killed in the lock; second, in 1873 on the Tay Bridge works, where six men who were below in the excavating-chamber lost their lives; third, in 1877 on the Alexander Bridge works, St. Petersburg,[1] when ten men were blown into the air and killed, and nineteen men below in the excavating-chamber were drowned, their bodies not being recovered till twelve months later. These three accidents, spread over a period of twelve years, were the cause of a loss of thirty-seven lives, which, compared with the statistics of the results of boiler explosions, show that air-lock accidents are much more disastrous; these considerations lead to the conclusion that this apparatus should receive as much attention in the way of periodical examination and testing as is applied to steam-boilers, an easy matter, as those now engaged constantly in testing steam-boilers hydraulically could equally well carry out the testing of air-locks in a similar manner. There should also be rules formulated for enabling the strains upon the air-lock shafting, &c., to be easily found. The Author intends publishing a series of calculations in reference to the strains upon the various parts of the whole apparatus in an early edition of the "Deutschen Bauhandbuch;" and he in this Paper enters minutely, by a series of equations, into the question of the strains around the opening made in the wall of the cylindrical air-lock for the door, based upon the principle that a cylinder with closed ends, under pressure from within, is mainly subjected to two strains, the one $P \times R$ acting circumferentially and tending to split the cylinder wall vertically, and the second $\frac{P \times R}{2}$, or half the intensity of the first, acting at right angles to it (or in a direction parallel to the axis of the cylinder), where P = pressure on area of wall and R = radius, from which is deduced that if the frame around the rectangular doorway be of insufficient strength, rupture will commence at the angles; and he advises that in all cases with either rectangular or circular doorways that, in addition to the framing at the side of the door, a ring of flat bar iron of a diameter equal to the diagonal of the rectangular-door, and with a sectional area of $\frac{3}{4} \frac{p \times R \times a}{k}$ (where a = height of door-opening, and k = unit strain) shall be riveted to the wall of the cylinder. This will take the main strains above alluded to. The pressure in addition to the above, acting upon the back of the door from within outwards, may be met by riveting an angle-iron ring on to the before-mentioned bar-iron ring.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxv., p. 286.

The Author suggests that adoption of certain rules (of which the following is an abstract), with regard to the working of air-locks and shafts might be of service, viz.: the iron to be of the best quality with a tensile resistance of 22·86 tons per square inch, and 21·6 tons per square inch with and across the fibre respectively; cast-iron may only be used for the tubes, which, if subjected to pressure from within, do not exceed 12 inches (30 centimetres) in diameter, and if from without 24 inches (60 centimetres) in diameter, and may not be used for those parts subjected to vibration; pipes of brass or copper may not exceed 4 inches in diameter; the coefficient of safety to be at least five times the strain; shaft-tubing, in calculation, to be assumed as subject to occasional pressure from without. A manometer to be provided in each air-lock, also a plate affixed to the latter, giving the name of the manufacturer, the working pressure for which it is calculated, and the date of the trial-test.

At every new installation and also at intervals of not more than twelve months, while in use on the same works, the whole apparatus to be tested under a hydraulic pressure of twice the intended working pressure. In testing, the full pressure shall be continued for at least ten minutes without producing signs of weakness, by the buckling of plates or escape of water other than in the form of dew. The result of this test to be entered in a register. An air-lock is, at the extreme, not to be in use for more than one thousand five hundred days, and in this amount the intervals between work are to be reckoned as one-fifth of the working time. During the second half of the above period the air-locks are to be used only in conjunction with the excavating-chamber, in which the pressure does not exceed two-thirds of that for which the air-lock was originally intended. As before remarked, after a period of one thousand five hundred working days, a lock should not under any circumstances be continued in use for high-pressure foundation-works, and a register should be kept by some responsible person upon the works of the number of working hours, and the degree of pressure from day to day. Regarding the shaft-tubing, this may be considered serviceable for a period of five thousand working days, and a full allowance made for the hours when not in work, as the strains to which it is subjected are of a less unfavourable character than those sustained by the air-lock.

In conclusion the Author especially urges the necessity of a careful register of the working of the apparatus, in regard to the particulars above mentioned being imperative. By this means, in the case of the apparatus at the completion of works being disposed of, those taking it over may be fully acquainted with the amount of wear and tear to which it has been subjected.

D. G.

Railway Bridges over the Elbe at Hamburg and Harburg.

By — Lohse.

(Zeitschrift für Bauwesen, 1885, pp. 79 and 177.)

The question of the erection of these bridges crossing the North and South Elbe was considered by the authorities in 1857, and the waterway and levels were approximately fixed, but the actual construction of the Hamburg bridge was not commenced until 1868, and the Harburg in 1869. Owing to the war their completion was delayed until December 1872. They are double-line (4 feet 8½ inches gauge) bridges, with footways outside the main girders.

The Hamburg bridge crosses the stream at nearly right angles, by three main openings, each of 315 feet span, and four flood-openings, viz., two on each bank of 70 feet span. The navigation, which is here only for river-craft, is carried on by a loop-canal, on the right bank, crossed by the railway between the bridge and the station by a "Schwedler" swing bridge with two openings of 41 feet 8 inches span.

The Harburg bridge crosses the stream by four main openings, also of 315 feet span, and six flood openings on the right bank of 96 feet 3 inches span, and on the left bank is a "Schwedler" swing bridge with two openings, each of 41 feet 8 inches span across a canal.

All the seven main spans of the two bridges are of similar construction, viz., combined arch and suspension girders as hereafter described. The four sides of the Hamburg bridge are ordinary arches, and the side spans of the Harburg bridge are framed girders with curved upper flanges. Both the North and South Elbe are tidal (6 and 4 feet respectively). The height from mean tide-level to under side of the bridge platform girders is in each case about 20 feet 6 inches. The river piers of both bridges are, at the height of mean tide-level, 75 feet 2 inches long and 19 feet 7 inches broad, lessening to a width of 18 feet 6 inches at level of bed-plates. At each end of the abutments and piers there is an arched portal, supporting the ends of the combined bow-girders. The Author in his design was governed by the following considerations, viz., the slight headway permissible, a desire to keep the river-piers as small as possible, and lastly, the adoption of a design which should present a generally pleasing effect. To meet these requirements it was decided to carry the railway on a platform dependent from a pair of girders, on a combined arch and suspension system, formed by two parabolically curved members, the superior arch in compression, and the inferior inverted arch in tension. They are rigidly connected together at their ends or springing, whereby horizontal thrust is avoided and vertical strains only brought upon the points of support, which latter are bedded upon the masonry piers of the before-mentioned portals at a height of 23 feet 9 inches above rail-level. The combined girder comprises, as before-mentioned, two members, curved in opposite directions, with their inner flanges intersecting each other at about 14 feet 5 inches from the points of support. Each of these members is 10 feet 3½ inches deep, and formed with an inner and outer (top and bottom) flange of H section, composed of two vertical ½-inch plates, 1 foot 6½ inches deep, and a ½-inch cross-plate 1 foot 6½ inches broad, connected together and also stiffened at the outer edges by angle-irons. These vary in section according to their position in the curve, and several sheets of drawing giving these and other details accompany the Paper. The bracing of the members is composed of braced pairs of tee-iron verticals and angle-iron diagonals attached to the inner and outer flanges by gusset-plates ½-inch thick. The clear span is 315 feet, but the length of each girder between centres of supports is 325 feet 3 inches. This length is divided into twenty-six bays, viz., one at each end of 14 feet 5 inches and the remainder of 12 feet 4 inches each. The depth at the centre of span, from the top or outer flange of the upper (convex) boom to the bottom or outer flange of the lower (concave) boom is 66 feet 3 inches. The lateral distance apart of the main combined girders centre to centre is 27 feet; the clear width between the girders, and also the width of the portal archway, is 24 feet 8½ inches, and the width of the bridge over all from centre to centre of outside balustrades of the footways is 37 feet 8½ inches. The tension bars connecting the upper and lower booms of the combined girder, at each 12 feet bay and their prolongations, viz., the suspending bars, are each made up of four angle-irons with diagonal bar-bracing, attached to the members by gusset-plates, and at their lower ends to the cross-girders of the railway-platform.

The cross-girders of the bridge-platform are 2 feet 4¾ inches deep, and the longitudinal rail-bearers 1 foot 5½ inches deep. There is also a girder running the length of the span on each side of the platform, immediately below the suspending bars, which is connected to each of the cross-girders and by transverse bracing to the rail-bearers. Each combined girder is supported upon a hinged fixed bed-plate on the portal at one end and upon a hinged movable bed-plate at the other, provided with six cast-steel rollers of the usual description.

Details of the plates and angle-irons in each bay of the combined girders is given. The combined girders with their members curved in opposite directions, are parabolic curves with a divergence near the points of support. A Table is given of the length of each ordinate, also the length of the diagonal bracing for each bay with their various angles, &c., followed by a series of other tables, in all seven, giving detailed lists of the strains upon the various parts of the structure. For purposes of calculation, the fixed load, viz., the weight of each main combined girder and the weight of the platform dependent from it, and also of one of the loaded footways (or the weight of the longitudinal half of the bridge superstructure) is assumed as 2·38 tons per lineal foot, and the rolling load as 1·9 ton per lineal foot; also it is assumed that this load is equally distributed on the upper or compression-member, and the lower or tension-member of the combined girder, and again that in each instance the load on each girder is divided equally between its inner and outer (upper and lower) flanges. The limit of strain upon the metal both in tension and compression per square inch of section is 4·64 tons. The weight of each combined girder together with the half width of the platform, when fully laden, is 1,400 tons, half of which is supported by each bed-plate. These bed-plates rest upon bed stones of basalt supported upon massive columns of masonry connected together by an arch of 24 feet 8 inches span, bridging the tracks and thus forming a portal as above mentioned.

The weight of iron in each span is as follows:—

MAIN COMBINED-GIRDERS.

	Prussian lbs.	Tons.	Tons.
Compression-members			188·98
Tension ,,			163·95
Stiffeners, supports, and bracing at ends			54·77
Tension-bars between compression and tension-members			11·00
			418·70
Platform including suspending bars below tension-member			167·60
			586·30
Bed-plates of main combined girders	(31,607) =	15·55	
,, ,, platform	(3,145) =	1·54	
Rail expansion joint	(3,166) =	1·56	
			18·65
			604·95

The provision and erection of the ironwork of the seven main spans, each 325 feet 3 inches, which amounted to 604·95 × 7 = 4,234·65 tons, including delivery of materials, erection and removal of the necessary staging, and all incidental work and transport, was contracted for and executed at a price of £19 10*s.* 3*d.* per ton (384 marks per 1,000 kilogram); this price was an average, and included cast-iron and steel. The cost of this portion of the work was consequently £82,629 15*s.*

The ironwork of the four side-spans at the Hamburg bridge, each of 70 feet 3 inches span, amounted to a total weight of 231 tons, and cost about £19 10*s.* 3*d.* per ton, or £4,506 12*s.*

The six smaller openings of the Harburg bridge, each of 118 feet 9 inches,[1] have a total weight of 530·45 tons, and cost £18 18*s.* per ton, or a total of £10,026 15*s.* (66,845 thalers).

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[1] These are stated to be 96 feet 3 inches (29·35 m.) span, in an earlier part of the original Paper.—D. G.

The cost of each of the "Schwedler" swing-bridges at Hamburg and Harburg, with the two openings of 42 feet 2 inches, and weighing 77½ tons, was £1,700. A summary of the cost of the various items, including foundations, masonry, ironwork, &c., for the two bridges, including side-spans and swing-bridges, is given.

	£	s.
The total for the Hamburg bridge is	104,209	15
,, ,, Harburg ,, ...	185,627	7
	£289,837	2

D. G.

Repairs to the Bridge at Kampen. By J. M. Telders.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1884–1885, p. 265.)

The bridge at Kampen over the Yssel is in four spans of 45 metres, a centre span with a double lifting bridge of 17 metres in the clear, and one viaduct opening in the left bank of 6 metres. The river-bed consists of sand-alluvium. The foundations of the piers and abutments, of masses of concrete, rest on clusters of timber piles driven down below the river-bed. From about 4 feet below mean water-level to the girder beds the piers and abutments are carried up in brickwork. The superstructure for the 45-metre spans is of wrought-iron; the system diagonal bracing of the first order without verticals. The lifting-bridges of the centre span are of the same material. The two centre piers as well as the abutments are ornamented with towers.

The bridge was opened for traffic in 1873. On the 7th of January, 1883, the fifth pier, near the right shore, showed signs of settling. It was found that during the high spate of that day, and those preceding it, the scour had undermined the foundation, and that the pier was going down at the upper cut-water, at the rate of about 6 centimetres every quarter of an hour, till it had gone down 88 centimetres. The girders across the spans on each side were warped by the uneven position of the pier; traffic became dangerous and was suspended.

Nine days after this accident the town council appointed a commission, who reported on the matter on the 21st of April following, and advised to repair the bridge by removing the sunken pier, and the rebuilding of another founded at a lower level. They estimated this work at £8,000. This money was voted on the 22nd of May, which was increased to £10,000 on the 23rd of June, on the condition that this work was to be finished in good order on the 1st of November.

On the 25th of July work was started by bringing two large barges under the superstructure of the fourth span. The lifting of this as a whole, and placing it on a timber staging on the right-hand shore was accomplished without accident the next day. The fifth span was lifted and placed in a similar position a few days later, and on the 30th of July a beginning was made with the demolition of the pier. As much as possible the brickwork was broken away by hand, and below water the concrete and remaining courses of brickwork were blasted by dynamite charges, partly placed in drilled holes, and partly let down on the old work. The quantity of dynamite estimated necessary for this work was 500 kilograms, or 1 kilogram per cubic metre to be removed. Only 62·5 kilograms were however used. On the 18th of August the last pieces were destroyed, and divers immediately commenced clearing the bottom of the heaviest pieces. The remaining lumps were removed by a dredger, and on the 11th of September a beginning was made with the driving of the new piles. These are fifty-eight in number, driven down to reach 26 feet below the river-bed. They were in on the 16th, when the timber tub for the concrete foundation block was immediately started by driving a ring of sheet piling of square balks with tongue and groove. On the 20th concrete of hydraulic lime was shot in this tub. This was finished on the 9th of October, and the brickwork of the pier was completed on the last day of the month.

In the meantime osier mattresses loaded with basalt were sunk round the foundation of pier No. 4, and the river-bed deepened and widened near the north abutment. The two temporarily removed superstructures had also been repaired and strengthened with vertical struts.

On the 7th of November the superstructure of span V was replaced in its former position; on the 10th of November that of span IV; and on the 1st of December the bridge was again opened for traffic. The total cost of the work reached 124,070 guilders, or £10,380.

Eight drawings accompany the Paper.

H. S.

Flow of Water through a One-Inch Lead Pipe. By G. Sacheri.

(L'Ingegneria Civile e le Arti industriali, 1885, p. 97.)

The length of the pipe was 1042·15 metres (3,419 feet); the gradient for a length of 102 feet was 1 in 10·5 and for the remaining distance 1 in 142·86. The pipe conveys water from a reservoir to a village, and was quite new when the experiments were made; its diameter was found to be 25 millimetres (0·984 inch). The head of water was 8·90 metres (29·2 feet). The discharge was found to be 0·35 litre (0·01236 cubic foot) per second, giving a mean velocity of 0·713 metre (2·338 feet) per second.

The formula $Q = 20 \sqrt{\frac{h D^5}{L}}$ (the metric equivalent of Hawksley's $4 \cdot 71 \sqrt{\frac{h D^5}{L}}$), gives the delivery as 0·18 litre

(0·00636 cubic foot) per second. The Author attributes the difference between the result of his experience and the formula to the perfectly smooth surface of the pipe and its freedom from granular structure.

Another experiment was tried with the mouth of the pipe closed by a thin diaphragm having a hole in it 5 millimetres (0·197 inch) in diameter. In this case the actual discharge was found to be 0·098 litre per second (0·00346 cubic foot), whereas the discharge should have been according to formula 0·154 litre (0·0054 cubic foot); whence it appears that when the velocity is very small the resistances in such cases as that under consideration produce a greater effect than is generally supposed. From these and other experiments it appears that the loss of head due to resistance in the tube is as follows:—

With mean velocity 0·713 metre (2·34 feet), 0·1996 metre (0·655 foot), 0·127 metre (0·417 foot), 0·037 metre (0·121 foot); the loss of head would be 8·86 metres (29·07 feet), 8·16 metres (26·772 feet), 7·23 metres (23·72 feet), 4·60 metres (15·09 feet) respectively. These results differ considerably from those which have been observed from pipes of greater diameter and shorter length, and the Author attributes the increase of resistance as the velocity diminishes to the air which escapes from the water and adheres to the sides of the pipe.

W. H. T.

On Tidal Phenomena on the Dutch Coast.

By T. F. W. Conrad.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1884–1885, p. 122.)

In continuation of former communications on very low ebbs in the Marsdiep at the Helder, on the 8th of November, 1881, and of the 14th of February, 1882, the attention is drawn to the low tides of February 22, 1885, on the Dutch coast.

In the Tijdschrift, 1884–5, page 27, will be found a recapitulation of the tide-gauge returns since 1851, over thirty-three consecutive years. According to this Table the mean, for this period, of high water is 22·3 centimetres + AP, of low water 93·8 centimetres − AP, and the average rise of tide therefore 1·161 metre. The diagram of the 19th of September, 1883, shows the average curve of the rise and fall at Marsdiep under normal conditions. The sea falls during 6 hours, rises rapidly for $3\frac{1}{4}$ hours, and then remains at high-water for about $3\frac{1}{4}$ hours.

During spring-tides the top of the wave shows two heads with a depression between them. During neaps the two heads fall together. This is probably due to the joining of the two high-water waves, "from the Channel" and "from round Scotland."

On the 22nd of February, 1885, the curves show two consecutive extraordinary low-waters with but a very slight rise between them, this even remaining 2 centimetres below the mean low-water level. The wind was S.E. to S. by E., and feeble. The tide-curves at Ymuiden, and other places on the coast show the same characteristics, but in a less degree. The exception in this case does not so much lie in the extraordinary lowness of the tide, which, during the period just mentioned, occurred on several occasions, as in the exceptional lowness of high-water. Another instance of a double ebb was observed on the 26th of February, 1853.

Mention is made of a similar low ebb on the 20th of July, 1672, It is, however, difficult to obtain data on this occurrence of sufficient precision to be of technical value at the present moment.

H. S.

Experimental Works for Improving the Fairway of the River Don. By L. Lubimoff.

(Ingener, St. Petersburg, May 1885, p. 209.)

The Author, after discussing the features of the Don generally, and pointing out the causes which contribute to the formation of sandbanks and other obstructions, proceeds to give a short history of the attempts made since 1876 to improve the navigable channel. He describes two or three forms of removable wooden groins which were tried, but with indifferent success, on account of the time required for their construction and the necessity of removing them early in the autumn, and points out that permanent groins made of stone, of which there is abundance along the course of the river, cannot be adopted in the first instance because of the uncertainty of the effect of a given line of groin on the current, and the danger of the loose stones being carried into the fairway.

Under such circumstances it was determined to try groins built up of sand-bags. In the year 1879, and again in 1882, the agent of the Volga-Don Navigation Company had rapidly and successfully improved the channel in two places by the proposed method, but it was not till 1883 that serious attempts were commenced for a scientific application of sand-bag groins by the engineers in charge of the river.

The Author proceeds to describe in detail the method of filling the grain-bags, made of bass-mats, and depositing them in the bed of the river. The sections of the groins form triangles with bases of from one to four sacks wide, laid lengthways, and from one to four sacks high, according to the depth of water and the strength of the current. As many as seven thousand five hundred bags had been laid in a single groin. Fifty sacks measured 343 cubic feet, and cost, deposited in place, £2 6*s*.

The Paper is illustrated by plans of three shallows which had been successfully dealt with, and by plates giving in detail the construction of the temporary wooden groins which were first used.

W. A.

The Artificial Removal of Ice in Dutch Rivers.

By J. van der Toorn.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1884–1885, pp. 125, 146.)

In the rivers and estuaries of the Netherlands the icefloes at first become fixed in the tidal reaches, the sea mouths remaining free. In the Zuiderzee and at the mouth of the Yssel, where the tide rises only 5 to 8 inches, the ice sets itself first at sea, and then in the river, where it packs from the mouth upwards by the floes drifting down upon it. To remove ice packing in this river artifically would not lead to practical results, and might be a cause of danger to the low river-side lands.

In the Nether Rhine, the Leck, and the Niew Maas, ice seldom becomes packed below the railway bridges, owing to the strong tides and the steam navigation.

During the last hundred years many systems have been tried to remove or destroy ice-fields, but no means have as yet been found efficacious under all circumstances.

The first trials made with this object on a large scale were made in 1845. Gunpowder petards were then used, but without good results. In 1861 trials were made with steamers. These were costly, but did not effect much.

In 1871 petards were used to break the ice-fields in the Merwede, and steamers employed to finish the work. An ice-pack in the Leck was attacked with a steamer; both trials led to failures.

In the winter of 1876 tugs assisted by torpedoes were used, and also two iron-clad rams utilized. It was found that these last were perfectly useless for this work.

In the winter of 1881, besides petards of gunpowder, lithofracteur was used. It was then found that charges of 10 kilograms of gunpowder were the most effectual.

The operations of the winter, 1885, gave no great satisfaction.

The conclusion arrived at is, that even the most powerful means hitherto employed to break up ice-fields and packs in large rivers are of no avail unless they are favoured by a strong continual thaw, and in that case they only hasten by a few days the natural course of events.

H. S.

The Construction of Quay-Walls at Rotterdam.

By H. A. van Ysselsteyn.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1884–1885, p. 15.)

The increase in the shipping at Rotterdam made extension of quayroom necessary. In 1852 a beginning was made in widening the Boompjes quays by building a wall in front of the then existing one. For the sake of economy, the usual pile foundation was dispensed with and instead of it a rubble-mound of basalt was made up to low-water level, and a brick wall erected on this at a cost of £8,166 for a length of 750 metres. The work was situate on the concave side of the river bend and rested on a stratum of about 16 metres thickness of bog; before it was completed a length of 70 metres slipped into the river. The repaired piece was built on a pile foundation which, however, could not be driven to satisfaction through the remains of the former riprap mound. The whole of this work continued to settle and lean outwards till in 1882, when part of it fell over, a beginning was made with the total renewal. This new wall stands from 7 to 16 metres in advance of the old one on a continuous pile-foundation. The piles are driven through the slope of the existing rubble mound which prevents under-scour. The floor supported on piles 1 metre apart centre to centre is from 10 to 18 metres wide, lies 70 centimetres (2 feet 4 inches) below mean low-water level, and carries on its outer edge the quay-wall. The great depth of water just outside the new work precluded the use of a cofferdam, and a sort of diving-bell was resorted to, for the construction of the timber floor and foot-courses of the wall. The bell is a rectangular box of wrought-iron, 44 feet by 22 in the clear inside, and a clear height of 7 feet 8 inches. The walls are double, the web-plates being 16 inches apart. The weight of the bell itself is 70 tons, and ballasted with 116 tons, or together about equal to the weight of the water displaced at a draught of 4 feet. When a greater submergence is required the double sides are filled with water ballast to an additional weight of 32 tons, and tanks placed on the top can contain another 118 tons of water.

The air-pumps are driven by a 5 HP. portable engine fixed on shore, and coupled to the bell by means of cast-iron pipes and india-rubber hose. The ballast-tanks are filled from the service-hydrants of the town-supply.

The work for which the bell is intended is: first, the clearing away of the old work below low-water level; secondly, the fixing of the timber floor over the piles after these are driven, and the laying of the lower courses of the wall.

After the old work has been cleared away down to low-water level the caisson is floated light over the desired spot, then grounded by allowing some air to escape, at the same time filling the water ballast tanks; after grounding air is forced in again before the men enter. Although air-locks are provided for taking materials in and out, these are seldom used; timbers and other objects are mostly brought in by passing them in under the sides. The bell was made by John Cockerill & Co. of Seraing for £2,500.

The walls now under construction consist of columnar basalt in Portland-cement mortar up to mean high-water level. Above this, the facing is of basalt with a backing in brickwork. The average cost per lineal metre is 650 florins (about £48 per yard).

H. S.

The Elbing Highland Canals. By — Von Fragstein.

(Zeitschrift für Bauwesen, 1885, p. 63.)

This system of canals, constructed between the years 1844 and 1860, connects the group of lakes around Mohrungen and Preussiche, Holland, at a height of about 328 feet above the Baltic, with the Drausen Lake, whence flows the river Elbing, emptying itself into the Frische Haff, on the Gulf of Dantzic. The whole length of the canal navigation and branches is 123½ miles, of which 28 miles is artificial, and the remainder lake and stream.

The Paper is principally a description of that length of the canal connecting the Drausen and the Pinnau lakes,[1] the latter situated at a distance of 10 miles from, and its waters originally at a level of 343 feet 9 inches (104·8 metres) above, the former. When the canal was first constructed, the water-level of the Pinnau lake was lowered to the extent of 17 feet 5 inches, thereby reducing the difference in level between the two lakes to 326 feet 4 inches. Commencing from the Drausen lake, the canal continues level for a length of 1¼ mile, and in the next 2·17 miles rises a height of 45 feet 3 inches. This difference of level was surmounted, in the first instance, by five locks, which have recently been abolished and replaced by an inclined plane. In the following 4·66 miles the remaining height of 281 feet is attained by four inclined planes.

The cost of original construction was £212,325 (4,246,500 marks), and if assuming it to have been spent entirely upon the artificial portion of the canal-navigation, which is 28 miles in length, would amount to £7,583 per mile (94,376 marks per kilometre). Of this outlay £70,000 was expended on the four inclined planes, exclusive of the earthwork, which latter cost £27,000, or an average of £24,250 for each incline. The total height surmounted by these five locks and the four inclined planes being 326⅓ feet, the cost of each foot of rise for the whole length of the canal amounted to

$$£\frac{212,325}{326\cdot33} = £650\ 12s.$$

The cost of maintenance of the whole system (including the lake portion) of canal and works between the years 1861 and 1875 averaged annually an outlay of £27 2*s.* per mile for the lake portion, and £120 4*s.* per mile for the artificial canal portion.

The construction of the fifth inclined plane (at New Kussfeld), in place of the five locks already referred to, was completed in 1881, but soon after a rupture of the embankment took place, and entailed a delay of two years before it could be finally brought into service. In the meantime the traffic was carried on through the old locks. The cost of this incline, together with the expenses of repairs, amounted to £45,000.

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[1] For a further description of these canals, see Zeitschrift für Bauwesen, 1861, p. 149.

Each incline is laid with a double rail-track, and on these tracks run the ascending and descending wagons transporting the canal boats. The motive-power in the case of each of the four original inclines is obtained through the medium of a water-wheel, and in the fifth, or that recently constructed, by a turbine, the intervening arrangement of winding-drum, ropes, and sheaves being practically the same in all instances. Sections are given, showing the gradients upon the fifth incline, at which the difference in level varies from 41 feet 5 inches to 47 feet 6 inches, dependent upon the height of the water of the low-level, which is the same as that of the Drausen lake. The principal gradient is 1 in 11·6, changing into 1 in 24 on the lower portion of the incline leading to the low level, and the dip from the summit (formed by a curve struck with a vertical radius of 370 feet radius) into the upper level basin is 1 in 24.

There are two parallel lines of rail at the inclines, and each of these lines is laid with a continuous through track of 10 feet 9 inches gauge, leading from the lower basin up to and over the summit, down into the bed of the upper basin. In addition there is laid, for about 130 feet (40 metres), at the commencement of the high-level and low-level inclines, secondary tracks, raised 1·25 foot (0·38 metre) above the main track, of wider gauge (12 feet) (3·65 metres) in the former instance, and of narrower gauge (9 feet 6 inches) in the latter case. These secondary differential tracks have an axis identical with that of the main track, and are for preserving the horizontal position of the boat-wagon after entering or until leaving the water.

The boat-wagon runs upon eight wheels, divided into two groups fore and aft; the distance between the centres of these two groups is 29·85 feet. There are two of these wagons at each incline. They are 65 feet 7 inches long over all, and constructed for vessels of 60 tons gross tonnage, or a total weight, including wagon, of 86 tons. The two wagons upon the fifth incline weigh together 52·0 tons, and cost £1,390. The rails weigh about 76 lbs. per yard. Both boat-wagons are in movement in opposite directions at one and the same time upon the up and down track. A rope is attached to each, which is wound or unwound upon a drum of 12 feet 4 inches diameter, actuated at the fifth incline by a turbine (at the other four by water-wheels). A third rope, of less diameter, is attached to the rear of each wagon, and is carried round pulleys at the foot of the incline, thus connecting them together and ensuring the maintenance of their proper relative positions, and enabling them to surmount the short incline leading from the upper basin to the summit. A Table is given of the lengths of the main and differential tracks at each of the five inclines, and also the differences of level surmounted. The latter vary from 41 feet 5 inches (Neu Kussfeld) to 80 feet 4 inches at Schönfeld. A detailed description is given of the water-wheels at four of the inclines, and of the turbine at the fifth incline. Each water-wheel is 27 feet 9 inches (8·47 metres) outside diameter, and 13 feet 5 inches clear breadth, and under an effective fall of 21 feet 7 inches is capable of developing 68 HP., with a water-supply of 37 cubic feet per second.

The turbine works under an effective head of 26 feet 3 inches, and is capable of making 140 revolutions per minute, with a periphery velocity of 26 feet 11 inches per second. The mean diameter of the buckets is 3 feet 5 inches.

The water-supply pipe is 3 feet 5 inches in diameter. Drawings of the turbine, &c., are given.

The ropes are of galvanized charcoal-iron wire. The back-rope is of $1\frac{1}{8}$ inch diameter, and the traction-rope of $1\frac{3}{8}$ inch diameter; the weight of wire rope at the fifth incline is 7 tons, and cost £200. The total cost of the metal work at the five inclines, exclusive of the wire rope, is as follows:—

	Tons.
Railway-tracks	637·5
Rope-pulleys and rollers	127·7
Boat-wagons	231·5
Hydraulic motors	155·3
Wheel-gear	141·5
Water-pipes	218·3
Rope-drums	130·5
Total	1,642·3

Owing to the small quantity of water required for working the motors, and the plentiful supply afforded by the lakes, a considerable amount now runs to waste, which might be made available for various industrial purposes or electric lighting.

D. G.

The Repairs to the Lock at Hansweerd on the Zuid-Beveland Canal. By J. van der Toorn.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1885–1886, p. 5.)

The small side lock at Hansweerd rests on a pile foundation supporting a timber floor, on which the walls in brickwork are built. The natural soil consists of fine running sand. In 1876 it was observed that the earthen backing of the lock-walls showed signs of sinking. This was attributed to water percolating between the brickwork and the made ground resting against it, forming channels and carrying the earth away with it. To stop this, sheet piling was driven on both faces of the retaining walls. At first no further sinking occurred, but in September 1877 the same symptoms reappeared, and then extended themselves to new spots. On careful investigation it was then found that considerable silting up had taken place in the canal-bed, just outside the lock-chamber. A hole was found in the apron of the lock-floor, and a hollow underneath the floor itself. This was at first filled in with puddled clay, but it had all been washed out again shortly after, and the sinking of the ground on both sides of the lock continued.

In order to discover the direction in which the leaks originated, coal tar was pumped down through a tube, near the apron at the upper side of the lock-chamber. This made its appearance at the lower side, having passed underneath the floor. This showed that continuous hollows existed under the lock-floor. An endeavour was made to fill these by forcing down a mixture of water and sand, but without avail. As soon as the water-level on either side of the lock gates differed in height the sand was driven out again. It was then discovered that the timber piling was attacked by the teredo-worm, and that even the floor was not free from it.

In 1882 it was decided to fill up the hollows with Portland-cement concrete, of one part cement to five of sand. With this object eight holes were bored through the lock-floor, and the concrete forced down through tubes, so as completely to fill the space between the original soil and the underside of the floor. During these operations air and gas-bells were seen to rise, and also some of the coal-tar came up which had been sent down the year before. As soon as concrete was observed to come up through the holes in the floor, the supply was stopped. It was found that 37 metres cube of concrete had been used for filling up, which now formed a hard mass underneath the floor. The work had been started on the 12th of September, 1882, and on the 1st of November following the lock was again ready for traffic. This method had been applied to avoid using cofferdams and the laying dry of the lock, which would have been greatly more costly than the expenditure for this work, which only amounted in all to £177.

Two drawings are added to the Paper.

H. S.

Sluices, Scour-Culverts, and Waste-Weirs for Irrigation Storage-Reservoirs. By G. Torricelli.

(Giornale del Genio Civile, 1885, pp. 74 and 134.)

In storage-reservoirs for irrigation there are three structures of the highest importance, namely, the outlet for the irrigation-water, that for scouring the bottom, and the overflow weir. For the first of these, two principal methods of construction are adopted: one, used principally in France, consists in placing a series of sluices at different levels in the inner face of the reservoir dam, and has the serious defect of requiring a number of apertures through the dam, which ought to be a solid and homogeneous structure. The other method, which is the one generally preferred in Spain, consists in constructing a single conduit, and connecting it with a well inside the dam, to which the water is admitted by a series of openings at different heights. In this case there is only one sluice, which is placed at the outer end of the conduit, and, when the reservoir is full, requires the application of considerable force to open and shut it, the maintenance of a uniform discharge under the high and varying head being a matter of great difficulty. The Author proposes a system which will retain the advantages while avoiding the defects of each of the others, and is applicable to reservoirs on a much larger scale (for which there is a demand) than any hitherto constructed in Italy. This system consists in the construction of a circular tower of masonry inside the reservoir, and adjacent to the dam, with sluices at different levels to admit the water, in connection with a culvert underneath the dam, constructed in rock. In the particular case illustrated, the height of the dam is 55 metres (180 feet), and the volume of water discharged is 13 cubic metres (458 cubic feet) per second, and the sluices are designed to maintain automatically this uniform volume, at whatever height the water may stand in the reservoir. The wall of the tower is 17 feet 9 inches thick at the top, and 23 feet at the bottom. Within the wall is a winding passage and stairs, 8 feet wide, for giving access to the various sluices. At six different levels in the tower there is a set of seven passages through the wall, each 3 feet 3 inches square, and in these the sluices are placed. The reason for having a number of passages, instead of one only at each level, is the difficulty of getting sluices of large size to deliver a uniform volume, and also the difficulty of constructing large sluices. At a distance of about 4 feet from the outer face of the tower, an arched recess is formed above the aperture to contain the arms which work the sluice. An iron screen, strong enough to resist the greatest water-pressure which can be brought to bear upon it, separates the recess from the passage. The sluice is made to fit the passage; when closed it stands vertically across it at a distance of 7 feet from the outside: it turns upon a horizontal axis at the top of the passage, and when fully open lies against the iron screen under the recess. It is made of sheet iron, strengthened by vertical **I** irons, two of which are carried up above the axis and attached to chains connected with counterweights formed of stone rollers, hooped with steel, which run upon curved iron bars, and so constructed as to balance the weight of the sluice, the friction and the water-pressure, so that, at the right moment, the sluice opens of itself to such an extent as to allow the calculated volume of water to flow through it. The conditions of equilibrium are investigated in the Paper, and formulas given for calculating the various parts. Experiments have been made with a model of one of these sluices. Hand-worked sluices may be substituted for them, and the methods of calculating them are given.

The torrential character of the Italian watercourses, and the enormous capacity required for reservoirs intended for irrigation, render it necessary to impound the waters of the autumn, winter, and spring, which are always heavily charged with suspended matter, which would quickly fill up the reservoirs with deposit unless precautions were adopted to prevent their doing so. The method of dealing with this deposit is one of the most serious difficulties in constructing these reservoirs. As it is necessary to impound these muddy waters, the system of carrying off floods along a by-wash cannot of course be adopted. The same difficulty is met with in Spain and in Algeria. The Author refers at some length to the latter country, and to the difficulties encountered by Mr. Calmels, who adopted the plan of blowing up the deposit by means of compressed air, and carrying it away in the irrigation-water.[1] In Spain a scouring-culvert is generally built under the dam, and the deposit washed through when necessary. One difficulty that has been found in working these is that, owing to the sluice being opened gradually, there is no sudden rush of water, which is required to start the deposit: another was that, upon one occasion, a sluice-rod broke (probably owing to the jamming of some solid matter under the sluice), and the whole of the water in the reservoir ran away. The Author, however, considers that these difficulties can be overcome, and that the scouring-culvert is the right method of removing the deposit.

Mr. Calmels considers that the mixture of deposit with the water would be rather an advantage for irrigation, but in Italy such water could not be applied to growing crops, and it would settle along the sides and bottom of the canal, and gradually fill it up, so that if the blowing system were used, the water before passing along the canal would have to be admitted into depositing-basins of considerable size. It is a well-ascertained fact that deposit in the reservoir always takes place first at the foot of the dam; and therefore, if that is kept clean by means of the scour culvert at its lowest point, no filling up can occur. In certain cases a constant flow might be allowed through the culvert, of sufficient force to prevent the formation of any deposit. Where this could not be done, the culvert might be opened with advantage whenever water was flowing over the waste-weir. In some cases it might be well to utilize the force of the water flowing through the culvert to drive machinery, by which any accumulation in the reservoir might be either stirred up, or dredged and brought to the surface. It is true that there might be some danger of the matter brought down being deposited below the dam, and interfering with the flow of the stream; but wherever, as would generally be the case, the mud possessed manurial value, there would be no difficulty in disposing of it. The Author therefore considers that the scour-culvert will completely answer the purpose of preventing the silting up of the reservoir, which was at one time thought to be a serious danger. He then describes a sluice suitable for this culvert, which will open quickly so as to allow a rush of water to act upon the deposit. The sluice opens and shuts vertically. Above and below it are cylinders with pistons connected to it by rods; these cylinders, by means of suitable pipes and taps, can be put in connection with the water in the reservoir, the pressure of which forces the pistons up or down, and so opens or closes the sluice. The necessary calculations are given in the Paper.

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[1] Vide Minutes of Proceedings Inst. C.E., vol. lxxi., p. 411.

In addition to the sand and mud which must be dealt with as described above, many of the reservoir feeders being swift mountain torrents, bring down masses of stone and rock, sometimes measuring 250 cubic feet or more. It is obvious that these must not be allowed to enter the reservoir. To stop them, dams must be built at convenient places across the bed of the torrent, which can generally be done at very small expense. Basins will be formed behind these dams, and will no doubt be filled with *débris* after a time, but the bed will be improved, and, if necessary, the dams can be raised or others built.

As the flow from these torrents is very large, it is necessary to provide in the main reservoir waste weirs and channels of large discharging capacity. Steep gradients will be necessary, and stairs cut in the solid rock and coated with cement. It is a curious fact that the rock, instead of being worn away by the action of the water, gets coated with a calcareous deposit which protects its surface, the explanation being that the violent rush of the water brings all its molecules into contact with the air, and the bi-carbonate held in solution is turned into an insoluble carbonate. It is found in Spain that the receiving culverts are not injured by the extreme velocity of the muddy and sandy water passing through them.

W. H. T.

Report of the Paris Drainage-Commission.

By J. C. A. Alphand and A. Durand-Claye.

(Annales des Ponts et Chaussées, 6th series, vol. x., 1885, p. 458.)

Paris, though provided with a large system of sewers and an adequate water-supply, is more subject to typhoid fever and diphtheria than other large cities of Europe. Alone amongst capitals, Paris has retained the cesspool system; whilst the closet arrangements, especially in the workmen's houses, are unsatisfactory. A commission, in 1880, reported in favour of irrigation as the sole method of purifying the Seine, and suggested the Achères domain as peculiarly suited for the purpose; but they recommended that the night-soil should not be discharged into the sewers, and proposed to carry it out of Paris by an iron sewer so that it might be separately treated. The municipality of Paris, in the same year, expressed the opinion that cesspools should be abolished, that all matters should be discharged into the sewers, and that the sewage waters should subsequently be purified by irrigation. The drainage commission decided in 1882 that the sewage waters of Paris in their present state, containing at least a fifth of the excrementitious matters, could be safely used for irrigation; basing their decision on the results of sewage farms abroad and the experience of Gennevilliers, and the purifying influences which result from aeration and dilution. They decreed that each lodging should be provided with a water-closet, with siphon trap and ventilating pipe rising above the roof, and supplied with not less than 2¼ gallons of water per head per day. They determined that cesspools should be abolished as soon as practicable; and condemned equally the tub and pail systems, and all other methods of receiving sewage which are exposed to leakage, which necessitate its remaining for a time in the dwelling, and its subsequent removal and treatment. Having decided upon the system of water-carriage for the house sewage, the commission eventually resolved, by a large majority, to recommend the discharge of the sewage into the excellent network of sewers which Paris possesses for the discharge of waste water, general refuse, and rainfall. They advised that these sewers should be made more efficient for discharging rapidly this additional refuse, by flushing reservoirs of water, each containing about 13 cubic yards and placed at intervals of about 820 feet, discharged instantaneously once or twice a day; by rounding off the angles; by reconstructing portions of the old sewers; by providing movable receptacles at the mouths of drains bringing down detritus from the streets and other heavy matter; and by employing regular gangs of men, aided by flushing-boats and flushing-trucks, for cleansing the sewers. These precautionary measures are rendered necessary by the tendency for banks of sand and silt to form in the sewers where the fall is slight; and the resulting stagnation might be very deleterious when the proportion of sewage has been largely increased. Besides advising additions to the outfall sewers, the commission recommended that the Seine should be shut out from the sewers in flood-time; and that the water should be lifted, at such periods, by additional pumping power at Clichy, so as to prevent any stagnation in the flow, which works have since been carried out. Finally they urged that the system of irrigation should be extended, both for dealing with the Paris sewage, and also for the sewage of towns on the Upper Seine and the Marne, to prevent the pollution of these rivers, as the purification of sewage water by land irrigation can be effected without any injury to the public health.

L. V. H.

Purification of the Seine.[1] By ALFRED DURAND-CLAYE.

(Annales des Ponts et Chaussées, 6th series, vol. x., 1885, p. 477, 10 plates.)

The sewers of Paris discharge into three main sewers. Of these, the intercepting sewer of the left bank, which crosses the Seine at the Alma Bridge, and the intercepting sewer of the right bank unite into a common outfall, and discharged into the Seine at Clichy below Paris four-fifths of the drainage waters of Paris, amounting to between 340,000 and 420,000 cubic yards per day. The third main sewer, collecting the waters of the northern and higher parts of Paris, discharged from 52,000 to 78,500 cubic yards daily into the Seine at St. Denis. The state of the river was so seriously affected by the discharge from these sewers, from Clichy down almost as far as Mantes, that the Government, after a series of investigations and experiments begun in 1867, decided to resort to the system of irrigation on the plains of Gennevilliers, and commenced operations on a small scale in 1869. This system has since been developed at Gennevilliers, on the left bank of the Seine opposite St. Denis; and now 1,500 acres are irrigated by water from the sewers, amounting annually to about 25,000,000 cubic yards in volume. It is proposed to extend the system of irrigation to some of the lands and woods of Achères bordering the Seine a little lower down, just above Andrésy; and arrangements are in progress for selling 3,040 acres of these lands to the municipality of Paris. The sewage water is brought on to the land by two distinct conduits. One conduit consists of an egg-shaped sewer, 5¼ feet high and 3 feet wide, which starts from the main, or departmental sewer at the chapel gate of the fortifications, and conveys the water of this sewer, by gravitation, down to the St. Ouen Bridge, where the water is carried across the Seine under the roadway of the bridge, in three cast-iron pipes 2 feet in diameter, and discharged, on the opposite side of the river, into a 3¼-feet concrete sewer forming part of the distributing system on the plains of Gennevilliers. This conduit has a total length of 4,070 yards, of which 3,611 yards are built in masonry, and 459 yards are of iron. The other conduit issues from the Clichy outfall sewer, about 380 yards above its outlet into the Seine. It is a circular masonry culvert, 7 feet in diameter and 967 yards long, and discharges into an aqueduct, 131 yards in length, which serves as a well for the lift-pumps and extends to the Seine. The pumping station contains three groups of centrifugal-pumps, worked by steam-engines, attaining together 1,100 HP. The first group of pumps, with an engine of 150 HP., was erected in 1873; and the second group, with an engine of 250 HP., in 1875–6. These pumps raise the water from the aqueduct and throw it into a cast-iron pipe, 3 feet 7 inches in diameter, in which it is conveyed under the footpaths of Clichy Bridge to the opposite side of the Seine, where it is discharged into a 4-foot masonry culvert belonging to the Gennevilliers irrigation system. The third group of pumps, with two engines of 700 HP., destined to furnish the supply of water for irrigating the Achères domain, can lift 275 gallons of water per second a height of 65 feet. At present they work alternately with the first two groups, and under these conditions can lift 396 gallons per second. A flood-gate with movable weir, recently erected across the outfall, prevents the Seine in flood-time from flowing up the sewers; and then the engines pump the sewage water into iron pipes which discharge into the river at the highest flood-level. This arrangement prevents stagnation in the sewers, and ensures the outflow of the sewage water during great floods. The two sewers already mentioned, which receive the waters after they have traversed the Seine, discharge their contents into a series of branch culverts, 21 miles in length, made of moulded concrete, from $3\frac{1}{4}$ feet to 1 foot in diameter, laid under the roads of the district. These sewers distribute the supply through a series of lateral glazed-earthenware pipes, furnished with screw-valves at their outlets, from which the water passes into rows of furrows by which the land is intersected between the ridges on which the plants grow. Five drains, having a total length of about 5 miles, were made in 1878–9 to drain the district. They consist of concrete culverts, 18 inches in diameter, perforated in places and surrounded by rubble; and the purified drainage water which they collect is discharged into the Seine. The total daily discharge from the main sewers, amounting to about 473,500 cubic yards, is equivalent to 74 per cent. of the water-supply and the rainfall at Paris. The water of the Clichy sewer contains 1·006 lb. of organic matter, including 0·040 lb. of nitrogen, and 2·394 lbs. of mineral matter, including 0·017 lb. of phosphoric acid, in a cubic yard. The water of the departmental sewer contains 1·236 lb. of organic matter, including 0·052 lb. of nitrogen, and 2·768 lbs. of mineral matter, including 0·020 lb. of phosphoric acid, in a cubic yard. The sewage water is warmer than the waters of the Seine in winter up to April, and colder up to October. In 1883, the total supply of sewage water was 23,019,150 cubic yards, of which 13,123,910 cubic yards were lifted by the Clichy pumps, and 9,895,240 cubic yards came from the St. Ouen sewer.

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[1] Minutes of Proceedings Inst. C.E., vol. liii., p. 208.

The letting price of the irrigated land, which formerly was 30*s.* to 54*s.* an acre, has risen now to between 146*s.* and 162*s.* an acre; whilst its selling value is between £162 and £194 an acre, and in some sales has attained from £324 to £356 an acre. Very large crops are gathered off the lands, such as eight hundred to one thousand six hundred cabbages, two thousand four hundred artichokes, or 40 tons of beetroot, per acre. The gross receipts obtained by the farmers vary between £50 and £160 per acre, and even more for some crops. Vegetables are chiefly cultivated; and eight hundred cows are fed on the grass and other crops from the irrigated land.

Experiments of warping and irrigating with large quantities of sewage water have been carried on, over a field of about 4 acres, since 1880 with very satisfactory results. The ground, which is clayey and has a surface layer of soil averaging 20 inches in thickness, was irrigated between November 1880 and December 1883 with a volume of water which is equivalent to a layer spread over the whole surface, 100 feet deep. The experiments are still in progress; the irrigation is carried on every two or three days, amounting to one hundred and fifty-eight days in 1883; and every year the distributing trenches, from 8 inches to 1 foot in width, are changed, so that a trench runs through the centre of the site of a ridge of the previous year. The sub-soil water-level is kept fairly low by the drains; and the noticeable purity of the water issuing from these drains has been verified by numerous analyses. The effluent water, indeed, contains less than 0·07 grain of nitrogenous compounds per gallon; and hardly 200 microgerms are discernible under the microscope in a cubic inch of the water; whereas, in the same volume, the Vanne water contains 1,000, the Seine at Bercy contains 22,500, and the sewage water 320,000 microgerms. Between the two last census returns, of 1876 and 1881, the population of Gennevilliers increased 34 per cent.; and the sanitary condition of the locality is perfectly satisfactory.

The cost of the works was as follows:—

	£.	£.
Pumping-station and collecting-conduits—		
Pumping-station	60,295	
Clichy conduits and St. Ouen branch sewer	29,270	
		89,565
Distributing works—		
Main and branch distributing-culverts	47,210	
Drains	13,375	
		60,585
Various accessory works—		
Experiments at Asnières	10,595	
Preliminary trials at Gennevilliers	14,864	
Sundry expenses	2,214	
		27,673
Total cost of the works		177,823

The cost of the irrigation works alone, for the 1,412 acres under irrigation in January 1883, amounted to £33 9*s.* per acre. The cost, however, involved in the extension of the irrigated area since 1877 amounts only to £20 8*s.* per acre; whilst if the canalization, now nearly completed, was utilized for the 3,025 acres capable of irrigation on the plains of Gennevilliers, the cost per acre would be reduced to £15 12*s.* The annual cost of working and maintenance amounts to £15,600. The cost of pumping up the water is 0·0823*d.* per cubic yard, and the cost of distribution is 0·0655*d.* per cubic yard.

L. V. H.

Hanneken's System of dealing with Domestic and Street-Refuse.

(Gesundheits-Ingenieur, 1885, p. 585.)

Two meetings of the Polytechnic Society of St. Petersburg have been devoted to the consideration of the plans, proposed by Hanneken, for the disposal of refuse by fire. For the purpose of dealing with waste materials of all kinds, they are divided by him into three categories: (1) house refuse, consisting of dust, dirt, and kitchen stuff; (2) excrement; and (3) street sweepings and snow, which latter contains the impurities of various kinds thrown upon the roads. This snow, or, more correctly speaking, the ice and frozen substances from courts and streets, is melted in a special vessel, so arranged that the water as it is formed flows away, leaving the solids to be dealt with along with the excrement. The apparatus employed for melting the snow consists of a capacious box with an inclined bottom, the top being enclosed by hinged doors, and this is sunk in any convenient space in the area or courtyard, the top level with the paving, and serves for the reception of the snow, as it is swept up. A perforated steam-pipe passes through the centre of the box, and steam is blown in to melt the snow; the water flows away, as fast as it is formed, into a second chamber, where the solid impurities sink to the bottom, while the clarified effluent escapes by a surface overflow, and before being discharged into the sewers undergoes a process of purification on a large charcoal filter. The excreta are collected in the following way: the downpipes from the closets are conducted into an air-tight receiver, to which also the foul water from the various sinks, &c., is conveyed. The solid matters settle at the bottom, the excess of water flows on to charcoal filters, and thence to the sewers. The charcoal after a fortnight's use for filtering is employed for fuel, and is replaced by a fresh supply. At the bottom of the excreta-chamber is a pipe by which it can be emptied, and for this purpose the Author employs compressed air, the requisite pressure being obtained from the town water-supply. The solids are conveyed in a 5-inch pipe to an evaporating-chamber in the destructor-furnace. This furnace consists of three divisions, situated one above the other; the lowest is the fire-chamber with movable fire-bars. Immediately above the fire-chamber are the cast-iron retorts, whose sizes are regulated by the number of persons whose refuse has to be treated. The flames play round the retorts, which serve for the evaporation and carbonization of the excreta. They are filled by pipes leading from the excreta-chamber, and the gases evolved are passed down to the back of the fire-chamber, and are there consumed. The third, or uppermost division of the furnace consists of a low vaulted drying chamber, in which the kitchen refuse and dirt of all kinds is dried, the gases being likewise conveyed down into the furnace, and there burned. The dried and charred refuse is allowed to pass ultimately into the fire-chamber by means of trap doors. This system has been introduced at the Palace of the Prince of Oldenburg and elsewhere, and the annual cost per head for disposing of all dirt and excrementitious matter is stated to be 5·50 marks (5*s.* 6*d.*) G. R. R.

Some Municipal Works at Rotterdam. By G. T. de Jongh.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1884–1885, p. 9.)

Slaughter-house.—The want of a public slaughter-house had long been felt; but it was only in 1875 that a beginning was made with the necessary works. At present the establishment is nearly complete. The buildings and yards, covering a space of about 12 acres, are fitted with all the most approved machinery and appliances for slaughtering animals, and the treatment of the meat. The water-supply is very large, and this rendered necessary a network of sewers of great extent. At present sixty-four head of cattle, three hundred hogs, and twelve to fourteen horses can be killed daily in the establishment. The original estimate was for about £46,000, but this has been exceeded.

Floating-dock.—The contract for this work was let in May 1881, and comprised two parts: the excavating of a basin on the left shore of the Maas opposite the town, and the ironwork for the dock itself. This last consists of two parts of similar cross-section but of different length, 90 and 48 metres respectively. The width over all is 27·40 metres, the width in the clear between the sides is on the floor 20·40 metres, at the top 21·40 metres. The bottom pontoon is 3 metres deep on the centre-line and 2·70 metres at the sides. The walls are 7·50 high, wide, 3 metres on the top and 3·50 metres at the dock-floor level. The bottom pontoon of the largest part is divided into twelve watertight compartments, that of the smallest in eight parts. The greatest intended draught is 10 metres. The pumps are the so-called "invincible" centrifugals of J. and H. Gwynne, with direct-acting engines with 20-inch and 15-inch pipes for the large and small parts respectively, and duplicate engines for each. The larger compartment can lift 4,000 tons; the smaller piece 2,000 tons besides its own weight. The eight mooring posts are fixed in blocks of concrete 5 by 5 metres square and 4 metres high, resting each on sixty-three piles, and covered with cast-iron caps, round which the mooring chains are laid. The contract price for the excavated basin was £14,742, and for the dock itself £69,560.

H. S.

Waterworks at Saint-Denis. By PAUL GUÉROULT.

(Mémoires de la Société des Ingénieurs-civils, 1885, p. 661.)

About the year 1835, at the angle of the Rue de Paris and the Chavigny Road, an artesian well was sunk, from which 120 cubic metres (26,400 gallons) of water were discharged in twenty-four hours, and supplied a public fountain, decorated by a statue of some goddess. The well hence derived the name of the "Artesian Well of the Goddess," which it bears to this day. In 1878, forty-three years later, the delivery had been reduced to 11 cubic metres per twenty-four hours, a falling off which was ascribed to the imperfect condition of the well. A new well, 131 yards in depth, was constructed in the neighbourhood of the old well, by Mr. L. Dru, at a cost of £1,040; and, as a security for success, the old well was filled up. The alluvium of the valley of the Seine, with chalk strata and sand and gravel beds, was traversed in sinking the pit, and the maximum delivery of water was attained at a depth of 115 yards, the water being supplied from the lowermost 9 yards of gravel. The well is constructed of cement concrete, lined with a copper tube 5½ inches in diameter, reaching down to the water-bearing strata, which is succeeded by a smaller tube of 3½ inches in diameter that penetrates the gravel. The concrete is encased in a protecting pipe of sheet-iron, about ⅕-inch thick, coated with tar, 13 inches in diameter for the first 50 yards, and 11 inches for the lower part. The water rises to a level of 14½ feet above the ground, and it is discharged at the rate of 168 gallons per minute, at a temperature of 52° Fahrenheit. It served to supply thirteen fountains, running day and night.

Two years later it was resolved to collect and utilize the water for the general supply of the district. Two collecting reservoirs, of from 200 to 300 cubic metres (44,000 to 66,000 gallons) in capacity, were constructed underground. From these the water is lifted by steam-power a height of 66 feet into a cylindrical sheet-iron reservoir, of 160 cubic metres (35,200 gallons) in capacity, erected on the top of a high tower. The total cost of the works amounted to £17,100, and that of the engines, boilers, and pumps to £1,800; together, £18,900.

The water from the well is delivered into the collecting-reservoirs through a cast-iron pipe, about 10 inches in diameter and 240 feet long. The reservoirs are filled during the night, and the contents are pumped off during the day, in addition to the water which flows into the cistern during the day, at the rate of 46 cubic metres (9,120 gallons) per hour. The reservoirs are constructed of millstone-cement concrete, arched over to a thickness of 18 inches, and covered with 2 feet of earth. The outside and inside of the reservoirs are coated with 1½ inch of Portland cement. They are readily cleaned out by flushing into the neighbouring sewer.

The water is lifted by two horizontal double-acting pumps, of about 16 inches in diameter and 32 inches stroke, drawing the water through $10\frac{1}{4}$-inch cast-iron pipes, with an air-vessel of about 70 cubic feet of capacity. They make 10 revolutions per minute, and are driven by intermediate gearing of 1 to 5, the engines making 50 revolutions per minute. There are two horizontal steam-engines, each of 12 HP., one to each pump. The effective force of 12 HP. is easily obtained with an admission of one-fifteenth of the stroke. The cylinders are covered with felt and mahogany. There are two boilers, of the elephant class, working one at a time, at $6\frac{1}{2}$ atmospheres; they have each $322\frac{1}{2}$ square feet of heating surface.

From the elevated reservoir two distinct services are supplied—the town, properly so-called, and the quarter of the Plaine-Saint-Denis, or Avenue de Paris, which commences at the fortifications and ends at the Porte de Paris. The water is supplied through a system of pipes, varying from 10 inches to about $1\frac{5}{8}$ inch in diameter, having a total length of 15,650 yards, controlled by sixty-two valves and cocks. The principal conduits form a circuit, from which the secondary pipes branch off. The joints of the pipes are on Petit's elastic system, in which a ring of indiarubber is inserted between the ends of the pipes. It costs 20 per cent. less than the ordinary socket and lead joints adopted in the City of Paris Works. On Petit's system of joint, two men can lay 220 yards of 8-inch pipe per day. On the other hand, the Petit piping is liable in a greater degree to damage by external causes.

The water is supplied to the inhabitants by sixty pillar fountains, regulated to discharge about 6 gallons per minute. Water is supplied, in addition, to eighteen public lavatories, and twenty-four firecocks, and for other purposes.

The working cost of supplying water gratuitously to the inhabitants of Saint-Denis, at the rate of about 6 gallons per head per day, amounts to £1,800 per year, comprising interest on capital, fuel, wages, and sinking fund. The engines and pumps, working twelve hours per day, can lift 394,200 cubic metres (86,724,000 gallons), at a cost of about 5*d.* per 1,000 gallons. The water company charge $42\frac{3}{4}$*d.* per 1,000 gallons.

D. K. C.

Sounding in Pipes by means of Metallic Potassium.

(Engineering News and American Contract Journal, Sept. 12, 1885.)

In connection with the preliminary investigation for the introduction of the improved-sewage scheme into Boston, Mass., an ingenious, if not novel, method was successfully tried for obtaining the height of water in small test-pipes driven into the ground. The purpose of the experiment was to test the effect of the pumping on the height of the underground-water.

About twenty small pipes were driven into the ground below the surface of the soil-rock, and measurements taken twice a day of the height of water standing in these pipes. The elevation of the top of each pipe being accurately determined, the exact distance to the water-surface was obtained as follows:—To the ring of an ordinary metallic tape, a small leaded plumb was attached by a wire hook; the top of the plumb was flat, and in a hole on one side of its centre was forced a cork, and in this cork a needle was fixed upright, eye downward, so that its point was just on a line with the bottom of the tape-ring. A small bit of metallic potassium was put on the needle-point, and the tape lowered into the pipe; the instant the potassium touched the water, it ignited explosively, and the flash and sound both gave the exact moment for reading on the tape the depth to the water-line.

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The Calorific Power of Illuminating-Gas. By Aimé Witz.

(Annales de Chimie et de Physique, 1885, p. 256.)

The investigation of the calorific value of a gas destined for illuminating purposes would be unnecessary if it were not at the same time employed for producing heat and work; but it may be assumed that, in the future, gas will be more employed for heating purposes and for motive power than for lighting, and the study of its calorific properties possesses therefore considerable interest. The Author is not aware that any accurate determinations have been made on this point. In Accum's treatise on gas-lighting, an experiment on the subject is described, which consists in burning the gas under a vessel containing a known quantity of distilled water, and observing the rise in temperature obtained by the combustion of a given quantity of gas; according to this author, 97 cubic inches of gas produced an increase of 50° Fahrenheit in 3½ lbs. of water; the calorific power per cubic metre would therefore be 10,000 calories, but this is manifestly too high. Mr. F. Fischer, a German philosopher, has recently adopted a similar plan for determining the comparative values of gas-burners: under the most satisfactory practical conditions, 0·974 cubic foot of gas was required to heat 2·2 lbs. of water from 32° to 212° Fahrenheit, and Mr. Lefebvre, one of the Engineers of the Paris Gas Company, has found the minimum consumption of gas for the same result to be 1·126 cubic foot. The available heat in gas furnaces would therefore be at most equal to 3,600 calories per cubic metre. If the coefficients of effect of the burner and boiler were known, the actual calorific power of the gas could be deduced from these figures, but this method of valuation would be very indirect, and consequently inaccurate. Mr. Dugald Clerk has also estimated 504,888 and 489,268 foot-pounds per cubic foot as the mechanical equivalents of Manchester and London gas, which correspond to 5,640 and 5,372 calories per cubic metre. It has hitherto been the general practice to accept 6,000 calories per cubic metre as the average calorific power of gas, but the results of the Author's researches indicate a considerably lower value.

The heat of combustion of gases has generally been estimated by their slow combustion in metallic vessels immersed in a calorimeter, but this plan is extremely tedious and liable to error. Mr. Berthelot introduced a more direct and more accurate method, consisting of the instantaneous combustion of an explosive mixture in a closed vessel plunged in the water of a calorimeter, and observing very accurately the elevation of temperature; this plan was adopted in its general principles by the Author. The explosion-chamber consisted of a nickel-plated steel cylinder, 2·36 inches internal diameter, and 3·54 inches high, the thickness of the metal being about 0·079 inch, and having a capacity of 15·56 cubic inches, with top and bottom covers screwed on so as to render the cylinder air-tight, a wire being passed through the top cover for the introduction of an electric spark, and the bottom cover fitted with a valve for filling and emptying. The calorimeter for the reception of the cylinder is nearly 4 inches diameter and 8 inches high; it requires 1·76 pint of water to submerge the cylinder; a means for agitating the water is also provided. A very sensitive and minutely divided thermometer is also required.

The cylinder is first filled with mercury, which is displaced by gas and air, or other mixtures, over the mercury bath, care being taken to reduce the mixture to atmospheric pressure by slightly raising the valve for an instant, and careful arrangements are described for accurately measuring the proportions of gas and air introduced. Before making the experiments, constants were carefully determined for allowances to be made for the absorption of heat by the various parts of the apparatus; these were made by direct experiments and verified by calculation.

As the result of a large number of experiments, made under varying conditions of time, temperature and pressure, and with gas manufactured at various works, the Author arrives at the following conclusions: The average calorific power of well purified illuminating-gas, as generally stipulated for by the concessions of French Gas-Companies, is about 5,200 calories per cubic metre (equal to 584 British units per cubic foot); the standard of 6000 calories, generally accepted up to the present time, being therefore too high. The calorific power of gas from the same works was found to vary from 4,719 calories to 5,425 calories at different periods of the year; but, comparing one works with others, the variation was not great if a sufficient number of trials were made. The necessary purification of the gas reduces the calorific value by more than 5 per cent. Contrary to what is generally supposed, the gas produced during the last hour of a charge is inferior in heating power to that obtained during the first hour, but it does not sensibly lose its calorific power by contact with water. The heating power of a gas may be increased 77 per cent. by carburation, but the gasoline employed becomes rapidly less volatile, and when reduced to one-fourth its volume, its enriching power is only

34 per cent. One volume of gas with six volumes of air produces complete combustion, leaving no appreciable trace of carbonic oxide in the products of combustion, but with excessive dilution the combustion is manifestly imperfect.

C. G.

NOTE.—The French calorie is equal to 3·986 English heat-units. It may be mentioned that the late Mr. F. W. Hartley gave, as the result of his experiments with 15-candle gas, a calorific value of 622 English units per cubic foot. See Transactions of the Gas Institute for 1884.—C. G.

Sulphur Compounds in Coal-Gas. By Dr. V. PETTENKOFER, Munich.

(Journal für Gasbeleuchtung, 1885, p. 825.)

During negotiations for the renewal of a gas-lighting contract for a town formerly supplied with wood-gas, and where coal-gas was to be substituted, it was considered desirable to guard against an excess of sulphur, beyond a stipulated quantity, by inserting a special clause in the contract; but in order to obtain an authoritative opinion whether it was of importance, from a hygienic or a chemico-technical point of view, for the gas to contain 8·74 or 21·85 grains of sulphur per 100 cubic feet, Dr. V. Pettenkofer was consulted, and expressed his opinion as follows:—

In unpurified coal-gas the greater part of the sulphur is in the form of sulphuretted hydrogen; this must be regarded as poisonous, even in small quantities, and therefore objectionable; it also attacks metals and colours, when it escapes unconsumed; it can, however, be completely removed by hydrated oxide of iron, and there then remains only a very small quantity of a compound of sulphur and carbon, which is no longer objectionable; sulpho-carbon compounds can in fact be mixed in large quantities with air to be breathed without being injurious, as is the case in india-rubber manufactories, where the fluid and volatile bisulphide of carbon is largely employed. Air containing bisulphide of carbon does not act like sulphuretted hydrogen on metals, and there is also a great difference in the quantities of sulphuretted hydrogen and the sulpho-carbon compounds contained in unpurified coal-gas. According to Buhe, gas, as it comes from the retorts, contains 1·38 per cent. of sulphuretted hydrogen, which is equal to 862·6 grains of sulphur per 100 cubic feet. Coal-gas, however, only mixes with the air of our living rooms in a burnt state, and sulphurous acid is produced by the combustion both of sulphuretted hydrogen and sulpho-carbon compounds; this, in a free state, is irritating to the respiratory organs, and acts, to a certain extent, injuriously on the blood, as well as attacking metals and colours; but if we assume the quantity of sulphur in unpurified coal-gas at 863 grains per 100 cubic feet, and in gas purified from sulphuretted hydrogen at 8·74, or 21·85 grains per 100 cubic feet, it is evident that the latter quantities cannot be of any importance, and, if unpurified coal-gas contained no more sulphuretted hydrogen than it does sulpho-carbons, its sulphur would never have been noticed. It was formerly thought that gas contained no other compounds of sulphur than sulphuretted-hydrogen, but of late years it has been found that, when entirely free from sulphuretted hydrogen, it still holds a small quantity of sulphur vapours, but they are only of scientific interest, for neither man nor things have been injured by them, their amount being too insignificant.

If the average consumption of a gas-flame is taken at 4·4 cubic feet per hour, then it must burn for eight hours to burn 35·3 cubic feet, and before 3·08 to 7·7 grains of sulphur are burnt, and, if there is one gas-burner in a room of 1,766 cubic feet capacity, then the sulphur in the gas will be diluted by at least fifty times its bulk of air, even assuming that during the eight hours absolutely no ventilation or change of air takes place.

To prove the non-injurious nature of the sulphurous acid produced by the combustion of the sulpho-carbons in coal-gas, seven lights were burnt in a very small chamber, having a capacity of not quite 35 cubic feet, and very slightly ventilated, the gas consumption being 30 cubic feet per hour of Munich coal-gas, free from sulphuretted hydrogen; the fouled air produced by this number of flames was tested by smelling and inhalation without a trace of sulphurous acid being detected, and it was also tested chemically for the same without any reaction being obtained. The very small quantities of sulphurous acid due to the sulphur compounds of coal-gas free from sulphuretted hydrogen hardly mix with the air of the room in a free state, but only as the salts of sulphurous acid, because there is always in the air of living rooms a certain quantity of ammonia, and also carbonic-acid salts, such as carbonate of lime, in the form of dust, which neutralize the acid. It is therefore certain that, from the hygienic as well as from the chemico-technical standpoint, it is of no importance whether coal-gas, free from sulphuretted hydrogen, contains 8·74, 21·85, or even 30·6 grains of sulphur per 100 cubic feet; gas-consumers can derive no benefit from this search after sulpho-carbon compounds, and can only be interested in knowing that the gas is free from sulphuretted hydrogen, and possesses the stipulated illuminating power.

C. G.

The Product and Exhaustion of the Oil-Regions of Pennsylvania and New York. By C. A. Ashburner.

(Transactions of the American Institute of Mining Engineers, Sept. 1885.)

The most important questions connected with the oil industry are those of oil supply, and possible ultimate exhaustion of the field. In January 1880, the Author prepared a chart illustrating these points, which was afterwards reproduced in the United States census Report on Petroleum and its Products. This has been brought down to the present time, and together with other matter bearing upon the subject, is included in the present memoir.

The oil regions proper are included in a belt of country about 160 miles long and 20 to 40 miles wide, which traverses the State of Pennsylvania from the Ohio State line on the south-west to the northern boundary of New York on the north-east, the most northerly district being included in the latter State. For convenience of description the region has been divided into six principal districts, whose areas and production up to January 1885 are as follows—commencing at the northern or New York end:—

District.	Productive Area.	Yield in Barrels.	
	Sq. Miles.	Total.	Per Square mile.
I. Alleghany . . .	31	15,000,000	419,000
II. Bradford	133	109,000,000	820,000
III. Warren	35	12,000,000	343,000
IV. Venango	65	55,000,000	846,000
V. Butler	84	69,000,000	821,000
VI. Beaver	16	1,000,000	62,000

The total production of the oil regions between 1859 and the end of 1884 has been 260,990,035 barrels of 42 (wine) gallons.

The oils, which vary in colour from light amber to dark green and black, are derived from beds of oil-sand which occur at different horizons in strata below the productive coal measures. Taking the bottom of the Olean conglomerate at Pottsville, conglomerate which corresponds to the base of the Millstone grit as a datum, the distance to the top of the Bradford oil-sand in No. II. district is 1,775 feet, while in the Warren district the oil-bearing levels are at 1,108, 1,300, 1,450, 1,625 and 1,830 feet respectively below the same bed. These strata are mainly fine-grained sands at the northern ends, but become coarser and occasionally contain pebbles towards the south-west. All are referred by the Author to the Chemung division of the Devonian period.

The oil of the Venango district, which was the scene of all the earlier developments, is derived from three principal sand-beds contained in a vertical depth of 350 feet. The top of the first or uppermost oil-sand is 450 feet below the bottom of the Olean-conglomerate. They are supposed to belong to the Catskill division of the Devonian. Although called sands they are essentially conglomerate of pebbles loosely cemented, and are more variable in character and yield in short distances than the more homogeneous sands of the Bradford and Alleghany districts. The conditions in the Butler district are generally like those in the Venango district.

The producing beds of the Beaver district are the Pottsville conglomerate, and the Berea grit of the sub-carboniferous series.

The strata are not uniformly productive over the entire areas of the districts; but the valuable locations are concentrated in so-called oil-pools, which vary very considerably in size. Thus in the Venango districts forty pools have been defined of a total area of 65 square miles, the largest covering 28 square miles. The Richburg in Alleghany district is of the same size. The Clarendon in Warren district 14 square miles, and the Pennsylvania portion of Smithsferry pool in Beaver district about 12 square miles. The latter pool extends into the State of Ohio.

That the general boundaries of the oil regions in Pennsylvania are now well established there is but little doubt, and that all the sands in which oil will ever be found in paying quantities are known seems quite certain, so that there can be no reasonable expectation of finding any new or extensive pools, comparable in area or yield with those already known. This opinion has been expressed by Professor Lesley and the Author's coadjutor, Mr. Carll, and the results of the studies of the Pennsylvania geological surveyors, and of the best informed practical operators, lead to the belief that the general confines of the territory are well defined, and that it is in vain to sink wells where none of the conditions characteristic of the oil regions proper are to be found. It must, however, be borne in mind that some of the individual oil-sands have as yet only been explored within limited areas. For instance, the profitable Bradford sand of the Alleghany and Bradford districts has not as yet been explored in the regions further to the southwest owing to its great depth, and the existence of profitable beds at higher levels; and as far back as 1878 a lower bed, the Smethport oil-sand, was discovered at a depth of 360 feet below the Bradford sand. It is a noted fact, however, that no two oil-sands, one immediately above the other, have been great producers within the same area.

The maximum daily production of petroleum was attained in July 1882 (105,103 barrels), since which time it has irregularly but steadily declined. In any particular year or month a material increase may be obtained by renewed activity in drilling wells or working over old ground; but such an increase can only be of limited duration, and a progressive decline over a series of years must be looked for. This is evidenced by the following figures, which show that in July 1883 there were 17,100 producing wells giving a daily yield of 3·8 barrels each; in July 1884, 21,884 wells averaging 3 barrels daily; and in July 1885, 22,524 wells, the average daily product being reduced to 2·5 barrels. These, with other collateral facts connected with the production, manufacture and consumption of the product, will make the exhaustion of the field a gradual one, and it is probable that the cost of production will be so great that the oil-men's occupation will be gone long before the supply has actually failed. It is hardly probable that the Japanese practice of mining 600 feet deep for a few gallons of oil daily will ever prove profitable in America.

H. B.

The Geology of Natural Gas. By C. A. ASHBURNER.

(Transactions of the American Institute of Mining Engineers, Sept. 1885.)

This Paper records a few general conclusions which have resulted from the observations made by the Author and others in connection with the geological survey of Pennsylvania over a period of ten years. The oil- and gas-regions of Pennsylvania are substantially one in a geological sense. The strata drilled through by the gas wells in the vicinity of Pittsburg are in a general way the same as those of the Beaver and other oil districts lying at distances varying from 8 to 45 miles from that place. Gas is a common associate of liquid oils, and the product of the so-called dry gas-wells, though supposed to be free from oil and water, generally yields one or both in small quantity, when subjected to great compression. The rocks of the oil and gas regions lie nearly horizontally in broad and almost imperceptible folds, whose axes are, roughly speaking, parallel to the escarpment of the Alleghany mountains. The maximum dip is about 70 feet per mile, but is generally much less. The vertical range of gas-producing beds is included in about 3,000 feet of carboniferous and Devonian strata extending from the Mahoning sandstone at the base of the lower barren coal measures, 500 feet below the Pittsburg coal seam, down to the Smethport oil-sand. There are three principal gas-horizons, the first being probably the representative of the Venango first oil-sand, 1,800 to 1,850 feet below the Pittsburg coal; the second, the Sheffield gas-sand, corresponding to the lowest oil-bed of the Warren district, 2,200 feet below the Mahoning sandstone; and the third, the Bradford oil-sand, 1,775 feet below the Pottsville conglomerate. In the adjoining states of West Virginia, Ohio, and Kentucky, natural gases have been obtained from lower horizons down to the lower Silurian limestones, but the most productive wells have obtained their gas from the Devonian sands. It is believed that the gas is not indigenous to the sand-rocks producing it, but that it is derived from the decomposition of organic matter contained in strata below. If this be so the amount of gas contained in any one sand depends upon the extent to which the rocks are cracked below it to allow the gas from the lower fossiliferous level to flow upwards, and the continuity of the covering strata, which, if much cracked above the gas-sand, would allow it to escape into the atmosphere and disappear. That the absence of both petroleum and natural gas in the plicated strata east of the oil regions is evident from the fact that these strata extend far beyond the areas in which oil and gas are found, and even within these latter the direction of certain productive flows is to be attributed to the jointing of the strata which, according to the Author, has resulted from contraction due to the secular cooling of the earth.

In selecting localities for boring gas-wells, the so-called "anticlinal theory" has been most generally followed. This supposes that the most abundant flow will be obtained on or near the summit of an anticlinal arch; but the Author does not believe that this is of universal application, as in many notable instances, which are given in detail, the gas has been found in synclinal troughs or in perfectly horizontal strata.

On the other hand some of the largest wells have been found on the crests of anticlinal axes; one of the most notable being the Sheffield well, drilled in 1875, which still supplies the town of Sheffield, Pennsylvania, with light and fuel. This is remarkable for the great pressure of the gas which rendered the drilling difficult. A vein of salt water, passed through at 418 feet, was allowed to leak into the well; the gas-sand, 45 feet thick, was struck at 1,350 feet, when the gas being suddenly relieved from pressure as it flowed into the well absorbed heat from the water so rapidly as to form a core of ice about 200 feet deep, which nearly stopped up the bore.

The average pressure of the gas in the Pittsburg district is from 100 to 200 lbs. per square inch. At Carnegie Brothers steel works 9 miles from the well, it is about 75 lbs. The highest pressure recorded as measured is 750 lbs. per square inch.

The composition of the gas varies from time to time. At Pittsburg the average is as follows:—

Carbonic acid	0·6
„ oxide	0·6
Oxygen	0·8
Olefiant gas	1·0
Ethylic hydride	5·0
Marsh gas	67·0
Hydrogen	22·0
Nitrogen	3·0

1,000 cubic feet, weighing about 40 lbs., are equivalent in heating power to about 55 lbs. of Pittsburg bituminous coal.

In the city of Pittsburg, one thousand five hundred dwellings, sixty-six glass-works, thirty-four rolling mills, and forty-five other industrial establishments are now being supplied with natural gas mainly for heating purposes; the amount of coal displaced being estimated at 10,000 tons daily. The estimated value of the coal thus supplanted in Pennsylvania and the adjacent States is given as follows:—

	£
In 1882	43,000
„ 1883	95,000
„ 1884	292,000

About two-thirds of the last amount (£220,000) represent the consumption in the Pittsburg district.

H. B.

The Working of Peat-Bogs. By — SCHACHT, of Oldenburg.

(Zeitschrift des Architecten- und Ingenieur-Vereins zu Hannover, vol. xxxi., 1885, p. 579.)

It is of extreme importance that special attention should be paid to the early works, as errors in laying out the drains can never be subsequently made good. In draining a very wet bog, having a total depth of, say, 5 to 6 metres (16 to 20 feet), it is usual to sink trial-holes, in the line of the main drain, to ascertain to what depth trenches can be sunk without entailing the collapse of the sides. These trial trenches should be from 5 to 6 metres in length, and from $\frac{3}{4}$ to 1 metre in breadth. In a very wet bog a maximum depth for the main drain of $\frac{1}{2}$ metre can only be obtained in the first year. During the second year the trench may be deepened to $\frac{3}{4}$ to 1 metre, and off-set trenches, or headings, at right angles to the main ditch, should be driven every 18 metres (three times the total depth of bog) to a distance of about 9 or 10 metres on either side of the main drain; and to such a depth as experience warrants, usually about 0·5 to 0·7 metre. They should not be narrower than 0·6 metre (2 feet), as this is the least width in which a man can work the second year.

The next year the trenches are excavated another $\frac{1}{2}$ metre. If all goes well, it becomes possible in the third year to deepen the main drain to $1\frac{1}{2}$ metre, with a width of 1 metre, and this done the preparatory work is at an end. The surrounding peat drains freely, and at the end of the third year, in order to provide an outlet for the more distant water endeavouring to find its way into the central drain, it is usual to cut so-called "parallel drains" on either side of the main drain, and at a distance from it of from 50 to 100 metres; these drains should be 1 metre square in section. In the fourth year the cutting of turf for sale may begin, and this can best be carried out in layers or "banquers," 3 metres wide and $1\frac{1}{4}$ metre deep. The total price, where labour costs 2 marks per day, is for the first three years' work 8 pfennige per cubic metre; the subsequent cost of cutting the peat, apart from turning, drying, &c., may be in the fourth year 15 pfennige, in the fifth year 20 pfennige, and in the sixth and following years 30 pfennige per cubic metre ($2\frac{3}{4}$*d.* per cubic yard). The above description has reference entirely to hand-work.

The Author then describes the various mechanical processes for dealing with peat, which depend upon a preliminary reduction of the same to a pulpy mass, and the subsequent spreading of the material on the bank to strain and dry. The simple "shredder" will deal with 100 cubic metres per diem, while the larger and more complicated machinery of Hodge or of Meeke and Sander will treat from 400 to 500 cubic metres per diem. The latter machine, known as the "turf-boat," will travel forward about 40 to 50 metres daily, and in so doing cuts a canal 6 metres wide and from $1\frac{1}{2}$ to 2 metres deep. The cost per cubic metre of turf excavated, pulped, and spread on the bank to dry, is set down at 13·2 pfennige (about 1·2*d.* per cubic yard). Details of the machine are given by reference to a drawing, and the nature and cost of work carried out from 1873 to 1882, in making canals through the Oldenburg peat bogs, is explained. Each cubic metre of bog yields approximately 100 kilograms of dry turf, which can be laid down at the market for 80 pfennige (about 7*s.* 11*d.* per ton); this about covers all the costs of production, and thus the formation of the canals entails neither profit nor loss.

G. R. R.

Comparison of the Annual Cost of Wooden and Iron Sleepers.[1]

By Gustav Meyer.

(Organ für die Fortschritte des Eisenbahnwesens, 1885, p. 194.)

To get at the annual cost for the provision and renewal of railway sleepers, the following must be considered: (1) The annual interest on the first cost; (2) The annual sum laid aside for depreciation; (3) The value as old material lessening the amount for depreciation; (4) The cost of labour and tools for changing the sleepers. The value of the various kinds of sleepers depends further on the cost of the attachments for the rails, on the cost of maintenance, and also on the influence of the sleeper upon the life of the rolling-stock. The first cost of the fastenings for wood sleepers is less than for iron sleepers, but the cost of renewal of them is greater in the former case, so that the annual cost for the fastenings is less in the case of metal sleepers.

There is not yet much ascertained as to the effect of the material of the sleepers upon the life of the rolling-stock; but it may be noted that in the statistics of tire-breakages on German railways in 1883, the number of breakages per 100 kilometres (62·1 miles) of line with wooden sleepers, metal cross-sleepers, and metal longitudinal sleepers are respectively 4·5, 3·7, and 1·7. In the absence of any satisfactory data the question of the influence of the road on the rolling-stock is left out of consideration in what follows: (1) The annual interest on the first cost C is taken at 4 per cent., or 0·04 C; (2) The annual allowance B to be charged for renewal after n years is (leaving out of account the value of the old material) $B = \frac{0\cdot04}{1\cdot04^{n+1} - 1\cdot04} \cdot C$; (3) The reduction of this (2) annual allowance due to the value of the old material of the sleeper is $\frac{0\cdot04}{1\cdot04^{n+1} - 1\cdot04}$ × the value of the old sleeper; (4) And similarly the annual amount to be allowed for the cost (about sevenpence) of changing a sleeper after n years is

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[1] It must be borne in mind that the facts and conclusions in this article refer almost entirely to lines where Vignoles rails are used.—W. B. W.

$$\frac{0 \cdot 04}{1 \cdot 04^{n+1} - 1 \cdot 04} \times 7 \text{ pence.}$$

The Author gives Tables based upon the above, showing the annual cost for wooden and metal sleepers of varying first cost and varying lengths of life. In constructing these Tables he takes the value of an old wooden sleeper to be 10 per cent. of its first cost, and of an old iron sleeper 40 per cent. According to these Tables a wooden sleeper of four shillings first cost, and twelve years' life costs 5·2 pence per annum (exclusive of maintenance) while an iron sleeper of eight shillings first cost, and thirty years' life costs 4·9 pence per annum.

Using the Tables, it is shown that the annual charge for a cheap oak sleeper creosoted at a cost of sixpence, and its life thereby increased to twenty years, is reduced from 4·8 pence to 4·2 pence, and generally that the annual charge for an oak sleeper is reduced by creosoting by about 0·6 to 0·8 pence per sleeper. The advantage of creosoting pine sleepers is still greater. The life of a sleeper, costing originally 2*s.* 11*d.* increased to 3*s.* 6*d.* by creosoting, is extended from six to twelve years, and the annual charge is reduced from 7·1 to 4·6 pence. For lines laid with flat-bottomed rails, as is usually the case in Germany, if wooden sleepers are used, oak is to be preferred, as it gives a firmer hold to the fastenings, while for rails carried by chairs, soft wood sleepers answer well enough, on account of the large bearing area of the chair.

The first iron sleepers were not successful. The attempt was made to use sleepers which should be as cheap as wooden ones, and the weight was consequently too small. The Vautherin iron cross-sleepers introduced in France, and on some German lines in 1864, weighed only 66 lbs. The fastenings had not hold enough; the holes widened out, and breakages ensued at the places where the rails rested on the sleeper. These effects were increased by the unsteadiness of the sleeper caused by its small weight, and the want of precaution against lateral movement. The defect was soon found out, and heavier sleepers of better design introduced. The Table on next page gives the weights and prices of iron sleepers used by various railway managements.

In connection with the life of iron sleepers, it is to be noted that they suffer less from atmospheric influences than wooden ones. Iron longitudinal sleepers laid in 1864 on the Brunswick Railways were found in 1882 to be but slightly affected by rust. Sleepers on the Bergisch-Märkisch line, after being laid eight years in badly-drained ballast, were found not to be more affected by rust than the rails. With the better and heavier sections now used, the breaking of a sleeper is so unusual as not sensibly to affect the annual charge. The life of iron sleepers has been variously estimated at from thirty to fifty years by those in favour of the system while opponents place it at not more than that of the best wood sleepers, or twenty years. This last estimate is, however, disproved by the more than twenty years' experience of longitudinal

NAME OF RAILWAY.	Weight of a sleeper.	Average price in Marks per Tonne.	Price of a sleeper.
	Kilogrammes.		Marks.
Reichs-Eisenbahnen in Alsace-Loraine	57·5 (126·8 lbs.)	131 (£6 14*s.* 1*d.* per ton)	7·53 (7*s.* 6*d.*)
Bergisch-Märkische Eisenbahn. Old Vautherin pattern	57·5	128	7·36
Bergisch-Märkische Eisenbahn. New Hilf pattern for main lines	44·5	128	5·70
Bergisch-Märkische Eisenbahn. New Hilf pattern for secondary lines	40·0	128	5·12
Links-Rheinische-Eisenbahn. Old pattern	35·0	143	5·01
,, ,, ,, New pattern	50·0	143	7·15
Rechts-Rheinsche Eisenbahn	48·4	127	6·15
Hannoversche Eisenbahn	47·0	160	7·52
Preussische Staatsbahnen (Magdeburg district)	54·5	152	8·28
Preussische-Staatsbahnen (various)	52·0	Average 143	7·44
Rechte Oder-Ufer Eisenbahn	46·0	174	8·00
Hessische Ludwigsbahn	44·5	131	5·83
Altona-Kieler Eisenbahn	46·0	..	6·71
Württembergische Staatsbahn	59·0	153	9·03

sleepers on the Brunswick Railways. The Tables above referred to show that iron cross-sleepers of 50 kilogrammes (110 lbs.) weight, costing 7*s.* 2*d.* each, only require to last twenty-five years in order not to involve an annual charge of more than 4·8 pence, the average for wooden sleepers on German railways.

W. B. W.

Steel Sleepers for Permanent Way. By J. W. Post.

(Schweizerische Bauzeitung, vol. vi., 1885, pp. 37–43.)

From a railway point of view the introduction of steel sleepers is good, because the average life of a sleeper is increased, the price of wood sleepers is lowered by the competition, and in industrial countries the making of a ton of steel-sleepers brings over the railways a traffic of about 10 tons of raw material.

The first trials of hard steel-sleepers were not encouraging, and some railway companies gave them up at once. Other companies laid test lengths on different systems, inspected them carefully and kept statistics of the cost of maintenance. Especially was this done on the Dutch and German railways, and with some success, and to these trials are due the results and experience now available.

Since the Bessemer, Thomas and Siemens processes have rivalled each other in producing mild steel, steel-sleepers have come much more into competition.

It is generally agreed that:—

1. The average life of good steel-sleepers is considerably longer than that of the best wooden ones.

2. The width of gauge is better maintained with steel sleepers.

3. The cost of maintenance of permanent way on steel sleepers remains almost constant after the second year, but on wood-sleepers increases constantly with age, so that the average cost of the latter is greater.

4. There are systems of fastenings on steel-sleepers which are at once safer and more easily maintained than those for wood sleepers.

5. A good steel sleeper should not cost more than from 125 to 150 per cent. of the cost of a wood sleeper.

6. The "old material" value of a steel sleeper is greater than that of a wood-sleeper.

If, for comparison of the relative cost per mile of steel and wood, account is taken of the manufacture, transport, laying, maintenance, interest (lately the low rate of interest has told in favour of the lasting material), and value as scrap, it is seen that there are few countries in the world where the exclusive use of wood-sleepers is really economical. For countries where climate and insects destroy wood sleepers in a few years, this is evident; but it speaks better for steel sleepers that in Holland, which produces no steel and gets wood sleepers very cheaply by sea, all the railway companies have introduced metal sleepers without any pressure from the Government.

The first metal sleepers were too weak; owing to a mistaken idea that they must be as cheap as those of wood, they were made only from 55 to 66 lbs. weight, and it was not clearly seen that they were weakest where the rails rested on them, because (1) the holes of the fastening reduce the cross-section, (2) the punching of the holes makes the metal round them more or less brittle, (3) the foot of the rail and the fastenings eat in time into the top of the sleeper, (4) with rational beating up of the ballast the moment of reaction of the ballast is a maximum at the cross-section where the wheel-load is applied, (5) the impulse of the moving-load is transferred directly to the sleepers at these places; and (6) in the various systems the material suffers most at these places during the forming of the 1 in 20 slope, to give the cant to the rail, whether that is done in the hot or cold.

The disadvantages of the sleepers being too weak soon showed, in the bending and shaking, the escape of the ballast and consequent expensive maintenance; also in the cracking of the sleepers lengthwise and crosswise where the rails rest. Some railways then introduced heavier sleepers (up to 165 lbs. weight), which lasted well, but were expensive. Various attempts to strengthen the sleeper by riveting or bolting to it plates for the rail to rest on, with or without the 1 in 20 slope, were unsuccessful because of the increased cost, or because the connection between rail and sleeper was less secure.

Latterly improvements in rolling machinery have rendered it possible to make sleepers of varying thickness, and having the 1 in 20 slope for the rail, the whole being done in the process of rolling. This improvement enables the metal to be disposed where most required, and effects a saving in weight of from 12 to 21 per cent. in the sleeper. The minimum cross-section of the sleeper is kept for about two-thirds of the length, while it is thickened under the rail and for a short distance on each side of it.

After the Netherlands State Railway Company had for some years laid test lengths of their main line on sharp curves and heavy gradients, with iron and steel sleepers of various designs, and carefully compared the cost of maintenance with that of similar lengths of line laid on new oak sleepers, they decided in favour of the mild steel sleepers, 8 feet 6 inches long, 9·25 inches wide over the extreme edges at the bottom, 2·52 inches deep for the greater part of its length but increased to 2·92 inches under the rail, and 3·23 inches at about 4 inches outside the rail, so as to give the inclination of 1 in 20 for the cant of the flat-bottomed rail which rests directly on the sleeper. Advantage was taken of the low prices of steel during the past two years to lay down large quantities of these sleepers on the Dutch, Belgian, and German lines of the company. One of the district engineers in his annual report for 1884, states that on a test piece of line 1,144 yards long, on a curve of 820 yards radius and a gradient of 1 in 83, no beating up of the sleepers was required for twenty-two months ending December 31st, 1884, and that the only maintenance required was one man for thirty-four days inspecting and tightening up the bolts. He states that the cost of maintenance of a steel-sleeper road, three-and-a-half years old, is the same as that of the same age laid on wooden sleepers, but that from this point the cost of the latter increases, while the cost of the former tends to diminish, owing to the consolidation of the bed. The following advantages are claimed for the sleeper (adopted from the St. Gothard Railway) by the Netherlands State Railway Company:—

1. It is easily packed with any kind of ballast, sand, gravel, ashes or slag.

2. The triangular toe which forms the bottom edge of the sloping sides of the sleeper prevents damage to the edge during beating up, and by lowering the neutral axis of the section gives additional stiffness. It also makes the section easier to roll.

3. It gives a broad surface for the foot of the rail to rest upon.

The weight of this sleeper is 104·7 lbs., whereas a sleeper of the same strength, with a uniform instead of a varying cross-section, would weigh 15 per cent. more. The last steel sleepers ordered (July 1885) cost at the works, including a two years' guarantee, not quite 6 francs each, or almost the same as an oak sleeper.

The Author proceeds to show by drawings how by giving them a varying cross-section the various forms of steel-sleepers now in use may be improved, including the Vautherin, Elberfeld, Prussian

State Railways, Rhenish Railway, and Austrian Railway patterns. The principle is applicable not only to sleepers for Vignoles rails, but the advantage of the local strengthening is even more apparent when, as in the case of Webb's sleepers as used on the London and North-Western Railway, a number of holes have to be made in the sleeper for the attachment of a chair to carry a bull-head rail; for in order that the rivets may hold for a length of time it is necessary that the plates should have a certain thickness.

There has been much discussion as to the relative merits of the Vignoles and bull-head rails, and German engineers who have been sent to England to report upon English permanent way, and its small cost of maintenance, have sometimes overlooked the fact that it weighs from 400 to 550 lbs. per yard as against 240 to 320 lbs. per yard the weight of the German line. For the same price as is paid for the English permanent way a line on the German system could be made just as solid and capable of resistance.

It has been objected that metal sleepers are not heavy enough to make a good permanent way. The secret of a good sleeper is, however, not weight alone, but depends also on—

(1) Appropriate form.
(2) A section with sufficient moment of resistance.
(3) A material not easily fractured.
(4) Sufficient bearing-area and length.

As to form, the closing of the end of the sleeper deserves special attention. In the early open-ended sleepers the 1 in 20 slope for the bed of the rail tended to drive the ballast out from underneath, while a sloping closed end, as now used by the Netherlands Railway Company, tends to drive the ballast in under the rail.

In order that the punching of the holes for fastenings may not have an injurious effect hard steel is now avoided, and the sleepers are made of mild steel (Bessemer, Thomas or Siemens) capable of resisting a tensile strain of 25·4 to 28·6 tons per square inch, with a minimum contraction of 30 to 40 per cent.

In case of anything leaving the line, also, mild steel is much to be preferred to hard steel for sleepers, as it is less liable to be broken.

Formerly there was anxiety as to the rusting of iron sleepers, but it is now known that this is trifling in the case of sleepers in use, so few railway companies use preventives such as galvanizing, steam oxidation, oxide of lead or tar. Only for the sleepers kept in reserve by the side of the line or for parts of the line where the atmospheric influences are bad (damp tunnels), or for sleepers for transport by sea is it needful to use tar or paint.

After many years of trial the Netherlands State Railway has decided to keep to screw-bolts as the means of attachment between the rail and sleeper. In 1865 they laid ten thousand iron sleepers of a now antiquated design on their main line near Deventer. The rails were fastened by four bolts, 0·42 inch diameter, to each sleeper. For the first time in 1883 it was necessary to renew two thousand of these forty thousand bolts, and the remaining thirty-eight thousand are still (August 1885) in use. This shows that the bolts 0·86 inch in diameter now used will be satisfactory. The bolt-holes in the sleepers are square except that the corners are slightly rounded.

The beating up of the ballast under steel sleepers must be so done that the middle of the sleeper is filled but not with compact ballast, while for a distance of 12 to 15 inches on each side of the rail the ballast must be well beaten up. It is also very important that during the first few months after the sleepers are laid, the road should have great and constant attention until the beds become consolidated. The cost of this ought to be considered as part of the cost of laying the road.

W. B. W.

The Earth-slips on the Dordrecht-Geldermalsem Railway.

By A. W. T. Kock.

(Tijdschrift van het Koninklijk Instituut van Ingenieurs, 1885–1886, p. 10.)

The Author gives a detailed account of the earth-slips on the newly constructed line between the Merwede river and the junction at Geldermalsem, particularly where heavy embankments had to be formed for inclines up to bridges and polder dams. In most places the line runs over very soft and peaty, water-bearing soil. Where peat and sand mixed were found the displacement sideways was found to extend over the greatest distances. Where the soil consisted of slippery clay and sand the corrugations or undulations formed were steeper. The Author is of opinion that in similar soils, where practicable, a channel should be dredged in the direction of the centre line, at least equal in width to that of the formation level, and down to the sandy stratum which everywhere in those parts underlies the upper stratum. This should be filled with sand, and also the body of the bank formed of it. Where the height of the bank is to be very great, side grooves running parallel to the centre line should be provided in addition, near the toe of the slopes, to be filled likewise with sand. He comes to the conclusion that to construct a dam with a core of lighter material leads to trouble, and is strongly to be deprecated.

The Paper is accompanied by two drawings.

H. S.

Phœnixville Tunnel, Pennsylvania Railroad.

By M. VAN HARLINGEN.

(Proceedings of the Engineers' Club of Philadelphia, May 1885, p. 3.)

The tunnel, recently constructed near Phœnixville on the Schuylkill division of the Pennsylvania Railroad, is cut through a ridge of hills nearly parallel with the Schuylkill River; in which advantage was taken of depressions on each side of the hill to shorten the approaches, as well as the tunnel itself.

The total length of tunnel excavated is 800 feet, or with façades 814 feet 9 inches; the clear width at the springing line is 27 feet; and the height is 19 feet 10 inches above the level of the rails. Work was begun in February 1883. There was 71,614 cubic yards of material to be moved before the headings could be started. About the 1st of June it was determined to sink at once for a heading, the project of sinking a shaft being abandoned. By this means work at the south heading was started on June 20th, 1883; and at the north heading on August 5th. By the 12th of August the headings had met. They were holed in one hundred and twenty working days, making an average daily progress of 6 feet, removing 39 cubic yards per day, at a cost of £1 1*s.* 10½*d.* per cubic yard; employing twenty-eight miners and seventeen "muckers" or labourers, in two shifts of eleven hours each. The work was commenced by steam-drilling, which, after the first day or two, was abandoned for hand-drilling. It was found necessary to timber the sides to the springing level, and for this purpose, as well as to use the tunnel temporarily to take the material for the way beyond, the American, or self-supporting style, was adopted, made like a wooden arch of 12-inch square stuff, in lengths of 4 feet. This was deemed the best adapted for the kind of material to be traversed, composed of layers of soft and hard red shale, with a few ledges of yellowish-brown sandstone, all of which showed fissures and cracks. None of the material was of a nature to keep a vertical position when exposed to the action of the air.

The benches in the tunnel were started, November 12th, 1883, at both ends, and were removed in one hundred and seventeen working days, when 17,011 cubic yards were removed, or 145·4 cubic yards, making 7 feet of progress, per day, with a daily force of one hundred and fifty-three men, besides seven horses, two locomotives, and fifteen dump cars; and at a cost of 10*s.* 5*d.* per cubic yard. The total excavation in the tunnel was 22,807 cubic yards, or 28·5 cubic yards per lineal foot. The footings or side walls were commenced at each end, in the middle of December 1883, in sections of 25 feet at a time, to allow for setting before the arch was started, the masonry being kept about 100 feet in advance of the brickwork. The arch was put in in sections of 20 feet. There are 887 cubic yards of masonry in the side walls, being 10·8 cubic yards as a day's work for five masons and ten labourers. There are 2,696 cubic yards of brick-arching, being 31·35 cubic yards, or a day's work for two foremen, nine bricklayers, and forty labourers. About 3,600 barrels of cement and 1,400,000 bricks were used in arching the tunnel. The arch was packed as it advanced with the materials taken from the tunnel, in order to distribute the load evenly over the haunches of the arch, and to fill vacancies between the timbers. The packing was done at night. In the façades there were 308 cubic yards of masonry, about 16¼ cubic yards being the average day's work for seven masons and fifteen labourers.

The heading was ventilated at the north end by means of a tin tube, about 4 inches or 5 inches in diameter, into the lower end of which a jet of steam was discharged, causing an upward current of air, and drawing in the foul air from the heading, which was replaced by outside air. At the south end a wooden trunk was used, 6 inches by 10 inches in cross section, with an exhausting jet of steam. This mode of ventilation was not very satisfactory, but it required only a short time to clear the air after a blast sufficiently to allow the men to resume work; and nothing further was attempted.

The cost of the tunnel was about £25,000; or per lineal foot, including façades, about £30.

D. K. C.

Railway Ferry across the Bodden between Stralsund and the Island of Rügen.

(Zeitschrift für Bauwesen, 1885, p. 357.)

The Island of Rügen lies off the Pomeranian coast of the Baltic, and is divided from the mainland by a strait (Bodden) of about 2 miles (3 kilometres) in breadth. There is a considerable traffic, especially during the summer months, when it is increased by a large number of tourists.

Formerly, the island during the winter months was occasionally altogether cut off from communication from without for weeks together; at those periods, when the coast-ice was sufficiently dense to prevent navigation by the steamers then at hand, but at the same time the main channel was insufficiently frozen or in a state of thaw, thereby preventing the passage of sleighs.

To remedy these inconveniences, after much consideration it was finally determined to transport the traffic by means of specially constructed steam ferry-boats and landing-stages, with bridges, free at their outer end to rise and fall as required, thereby permitting railway wagons to be run on and off a track laid on the vessel's deck.

The ferry connects Stralsund and Altefähr, and was opened for traffic in July 1883. The extreme rise and fall of tide is 8 feet, and the depth of the strait for the greater part of its width does not exceed 9 feet below mean sea-level (datum).

On the Stralsund side the approach to the ferry is on a gradient of 1 in 90 falling to the abutment, on which is hinged the moving bridge, where the rail-level is 1·45 foot above high water, or 5·30 feet above datum. This suffices for all but very exceptional tides.

The landing-places are so arranged that the ferry-boats can be brought alongside at exactly the same point and in the same relative position (excepting as regards level) at all states of the tide. This is effected by the angle formed between the main landing-stage and the guide jetty being so shaped as to form a recess exactly corresponding to the bow and forward portion of the ferry-boat.

The hinged wrought-iron framed moving-bridge, when not in service is suspended at its outer end from the cross-head of a wrought-iron "Portal crane." The weight of the former is counter-balanced by weights rising and falling in the interior of the Portal standards, and is governed by an ordinary winch worked by two men.

After the ferry-boat has been made fast alongside, the bridge is lowered, and rests on a bed-plate at the bow of the vessel, and is temporarily secured to the latter by bolts. The rails laid upon the deck of the vessel are thus brought into connection with those upon the bridge. The extremes of the variations in the gradient of this bridge due to the difference of tide-level are an up grade of 1 in 15, and a down grade of 1 in 13¾.

The main landing-stage is 23 feet 9 inches broad, and with a deck 5·87 feet above datum. It is of ordinary raking timber-pile construction, with a station-building and waiting-sheds. The depth of water at mean tide (datum level) is 10·5 feet (3·2 metre).

At Altefähr the arrangement is practically the same as the above, excepting that the inclined approach to the hinged bridge is on a gradient of 1 in 45.

Details are given of the hinged moving-bridges, each formed by two wrought-iron framed girders with curved lower flanges; their depth at the centre is 5 feet 7 inches; distance apart centre to centre, 5 feet 11 inches; length, 65 feet 8 inches; and they are calculated to carry a 40-ton tender-engine; the platform is formed of planking 11 feet 10 inches broad and 4½ inches thick, upon which is laid the rails and two longitudinal guards of oak to prevent derailment.

At its landward-end the bridge is hinged on cast-steel trunnions, working in cast-iron bearings, carried on a brickwork abutment with piled foundation.

The ferry-boats, of which there are two, are built, with a special view to encountering ice, with twin screws, and the fore keel rising with a gradual curve to the water-line, above which the cutwater is vertical.

The speed against the wind is 9·3 miles per hour, and draught when fully laden is 4 feet 7 inches forward and 5 feet 3 inches aft. The length is 114 feet 10 inches, breadth 24 feet 3 inches, and gross tonnage 300. The engines (surface-condensing) are 180 indicated HP., and consumption of coal for the above speed is 4½ lbs. per HP. per hour.

The vessels are constructed of wrought-iron, and divided by four watertight partitions. There is space for two hundred and fifty persons on deck (or four hundred if there are no railway wagons), and fifty in each of the four cabins. The bow and stern compartments serve for water ballast, and by filling or pumping these out by pulsometer, either end of the vessel can be raised or depressed, when breaking through ice or on making fast alongside the landing-stages at extraordinary states of the tide; it is also of use for depressing the stern, and thereby increasing the steering-power, when encountering strong winds under light lading. On the deck, fore and aft, is laid a rail-track sufficiently long to accommodate three or four wagons. Laden passenger-carriages are not transported, as, owing to the steepness of the inclines and the hinged bridges, there is not a sufficient degree of security.

The Paper is illustrated by four sheets of drawings.

The cost of the steam-ferry and works, exclusive of the station-building, were as follows, viz.:—

		£.		Marks.
Two ferry boats and berths		11,500		230,000
Landing place at Stralsund		1,150		23,000
,, ,, Altefähr		1,500		30,000
Two iron bridges, including portal crane	each £450	900	each 9,000	18,000
Dredging in Stralsund Harbour		600		12,000
,, Altefähr		3,700		74,000
Waiting sheds at Stralsund		350		7,000
,, Altefähr		650		13,000
		£20,350	Marks	407,000

D. G.

Railway up the Corcovado at Rio de Janeiro.

(Schweizerische Bauzeitung, vol. vi., 1885, p. 28.)

The granite cone of Corcovado rises close to Rio de Janeiro, whose suburbs extend to its foot. Being 2,336 feet high and isolated it commands a superb view. To render this accessible to tourists a rack railway has been made. The work began in March 1883, and the first section (2,959 yards) was opened in October 1884; the remainder to the summit is now completed. It starts in the suburb of Larangeiras, 121 feet above the sea, and rises to 2,198 at the foot of the actual peak. The following are the chief particulars:—

Length	4,144 yards.
Width of gauge	1 metre.
Least gradient	1 in 25
Maximum ,,	1 ,, 3·3
Minimum radius	6 chains.
Length on straight	2,834 yards.
,, curve	1,310 ,,

The line ascends steadily. The rails are of the Vignoles pattern, of Bessemer steel, in 29 feet 6 inches lengths, and weighing 40·3 lbs. per yard.

The rack-rail, which is identical in design with that of the Rigi railway, weighs 112·9 lbs. per yard.[1] The sleepers are 5·9 feet × 6·3 inches × 5·9 inches, and are of native wood.

Stations.	Distance from commencement. Yards.	Height above Sea-level. Feet.
Cosme Velho	0	121
Sylvestre	1,219	715
Paineiras	2,958	1,522
Alto do Corcovado	4,144	2,198

The chief works on the line are numerous retaining-walls, a wooden viaduct, two plate-girder bridges over deep clefts, and a wrought-iron viaduct of three spans (total length 426 feet) over the valley of Larangeiras. This viaduct is in plan on three different straights to accommodate the S curve of the line. It is on a gradient of 1 in 4. The two middle piers are wrought-iron framed pyramids, carried on granite-masonry beds. The rolling-stock consists of two locomotives with inclined boilers, two carriages to hold fifty passengers, and two goods vans. The ascent takes fifty-five minutes.

W. B. W.

The Narrow-gauge Railways of the Lake of Lugano.

By E. Olivieri.

(Il Politecnico, 1885, p. 231.)

The object of these two lines is to connect the Lake of Lugano with that of Coma on the east, and the Lago Maggiore on the west. The route of the former presented considerable difficulties. At a distance of about 1¼ mile from Menaggio (the terminus on the Lake of Lugano), is a summit 610 feet above the lake. There is a valley leading towards the summit; but, besides involving a gradient of 10 per cent., this valley is only a few yards wide in places, and is bounded by sides of vertical rock. This route being impracticable, several others were studied, and it was finally decided that on leaving Menaggio the line should ascend the hill-side bounding the lake, at right angles to the direct course, for a distance of 3,000 feet. At this point a reversing station is put in, and the line is brought back nearly parallel to its commencement to a point nearly above Menaggio, and then turns round into the valley, reaching the summit by a route of 2½ miles, thereby reducing the mean gradient to 4½, and the maximum to 5 per cent. The minimum radius of curves is 164 feet; there is a tunnel 360 feet long, and a considerable number of bridges of moderate size. From the summit the line descends with gradients of 4 per cent., sharp curves and numerous bridges, to Porlezza on the Lake of Lugano, the total length being 7½ miles. Upon the second line, from Ponte Tresa to Luino, also 7½ miles long, the ground consists to a large extent of micaceous deposits, yielding a large quantity of water, and its nature is very treacherous. The line was therefore carried for a considerable distance close to the stream, piles, retaining walls and pitching being required for its protection. A great number of small bridges were required to cross the numerous mountain-torrents, and sharp curves and gradients were necessary.

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[1] Minutes of Proceedings Inst. C.E., vol. xxxvi., p. 103.

The gauge of these lines is 2 feet 9¼ inches; formation width, exclusive of ditches, is 11 feet 2 inches; width of top of ballast, 6 feet 7 inches. The rails (of the Vignoles pattern), are of steel, 4½ inches high, in lengths of 29 feet 6 inches, weighing 45½ lbs. per yard. The sleepers are 5 feet 3 inches × 5 inches × 6¼ inches, and there are eleven to each rail. A good deal of difficulty has been found in keeping the rails to gauge, and the sleepers are now supplemented with wrought-iron tie-rods at the joints, on straight lines, three to a rail on curves of more than 230-feet radius, and five to a rail in sharper curves. The minimum length of straight between reverse curves is 45 feet, reduced to 23 feet by the parabolic commencements and ends of the circular curves. The superelevation of the outer rail in curves of 164 and 230 feet radius was, after several trials, fixed at 8 centimetres (3 inches), and this has to be gained on the parabolic arc, so that the two rails are level on the 23 feet of straight. The safety of the trains depends to a great extent upon the accuracy with which these points are attended to, particularly in coming down the 5 per cent. gradient, when the weight of the engine rests largely on the trailing wheels, leaving the leaders almost unloaded. Guard-rails are used on the 164-feet curves, but are apt to become a source of danger, either by stones getting fixed between the two rails, or from the wheels being liable to mount the guard rail if the gauge is not quite true.

In designing the engines it was necessary to comply with certain government regulations. The rigid wheel-base was limited to 6·1 feet, and the weight per axle to 6 tons.[1] It was therefore decided to adopt a six-wheel coupled engine with a bogy-truck. The bogy, however, could not be put in front of the engine and was placed under the foot-plate, and the engine is to be driven backwards. The area of the fire-grate is 7½ square feet, the total heating-surface 484 square feet. The diameter of the cylinders, 12 inches, and stroke 15¾ inches, the diameter of driving wheels, 31½ inches, the total weight 21½ tons, of which 4½ tons are upon the bogy. Upon the worst curves and gradients on the lines, these engines will draw a train of about 25 tons, at a speed of 6½ miles per hour, and upon straight and level portions of the line the speed is 18½ miles.

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[1] Eight tons per axle has since been allowed on the Avezzo-Fossato line of similar construction.

The carriages are first and second class, but present no special features of interest. High-sided open goods wagons have recently been adopted capable of carrying 10 tons. The length of body is 24 feet 6 inches, length over buffers, 31 feet 10 inches, there being platforms at each end of the body for brakemen. They are carried on two bogies, and pass round the sharpest curves with surprising facility; they are very steady in running, and as their load is the same as that of an ordinary wagon, they are very convenient for transferring goods to the broad-gauge lines. Their dead weight is 4½ tons.

The cost of the Menaggio-Porlezza line was £5,800 per mile, and of the rolling stock £1,224 per mile. That of the Ponte Tresa-Luino line was £6,100 per mile, and of the rolling stock £1,025 per mile. Both are single lines. The cost of financing, and the taxes paid to the Government are not included in the above sums. The Author considers that 4 feet 8½ inches gauge lines would have cost three times the money.

The Paper contains detailed descriptions of portions of the work, and estimates of the carrying capacity of the lines, and is accompanied by a series of drawings.

W. H. T.

The Wire-Rope Railway on the Superga, near Turin.

By — Grüber.

(Wochenschrift des österreichischen Ingenieur- und Architekten Vereines, 1885, p. 297.)

The Superga is an isolated group of hills east of Turin, from the top of which there is one of the finest views south of the Alps. To bring it easily within reach of visitors a wire-rope railway was made to the top (2,224 feet), in connection with the steam-tramway lines, and opened in May 1884. It is constructed on the Agudio system.[1]

Between the upper and lower stations, which are about 2 miles apart, there is an endless rope running quickly, kept in its course by the side of the single-line railway by guide-rollers on the ground, the return-rope being carried by rollers upon stone piers. The rope is passed round a driving-wheel on the drawing-car, the power being transferred from this driving-wheel to a toothed wheel working in a rack between the rails.

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[1] Minutes of Proceedings Inst. C.E., vol. lx., p. 380.

The engine-house is at the lower station, and contains two engines of 250-HP., which drive the rope and make 55 revolutions per minute. The rope consists of forty-eight steel wires, and is 0·87 inch in diameter. It passes round four drums, each 13·1 feet in diameter, as well as the two driving-wheels of the drawing-car. The drum at the lower end of the line is carried on a movable trolly, which is kept in position by a counterweight, and thus keeps the rope at an even tension. To correct greater differences of length the rope is passed three times spirally round the drum at the engine-house, and again in the same way round a third drum fixed on a trolly, which can be moved at right angles to the direction of the rope. The fourth drum is at the top of the mountain.

The line of rope which drives the car runs on small rollers, while the return rope is carried by rollers on the top of stone piers. These rollers are 3·28 feet in diameter on the straight portions, and from 7·5 feet to 8·2 feet diameter on the curves, where also they are suitably inclined. The minimum radius of the curves is 328 yards. The driving-rope travels at a rate of from 39 to 43 feet per second, and the drawing-car at from 7 to 10 feet per second (4·5 to 6·7 miles per hour). This reduction of the speed from the rope to the car allows of a rope of the light section used. The drawing-car is always kept on the down-hill side of the train, and is capable of pushing a load of 35½ tons (including its own weight) up a gradient of 1 in 5.

On the downward journey the rope is kept at rest, and the speed is controlled by a brake. By unfastening a coupling the car can be brought to rest on the upward journey. There are two emergency-brakes for use on the downward journey. The engine-house is in telegraphic communication with the drawing-car.

The Paper gives drawings of the plant and works.

W. B. W.

Steam Tramway from Hietzing to Perchtoldsdorf.

(Deutsche Bauzeitung, 1885, p. 433.)

This tramway, the first constructed in Austria, for working by steam power, was opened for traffic in October, 1883. It commences at Hietzing, a suburb of Vienna, and follows the course of the public road to its terminus at the Brunnengasse, Perchtoldsdorf.

Diagrams are given showing cross-sections of the tramway and public road, where the latter is bounded by houses, and also of where it passes through the open country. In the first case, the track is kept close to the footway, the average distance between the latter and the nearest rail being 1 foot 11 inches, except where the roadway is very narrow. The width of the tramway-formation is 8 feet $2\frac{1}{2}$ inches, viz., 4 feet $8\frac{1}{2}$ inches for the gauge, and cesses of 1 foot 11 inches and 1 foot 7 inches on each side. The tramway is separated from the footpath by a line of curbing, and the rail-head being higher than the level of the roadway (except where crossing lateral roads), the tramway-formation is not used for public traffic. The width of the roadway on the first portion averages 46 feet, deducting from this the tramway and the footpath, there remains from 23 to 26 feet (7 to 8 metres) for public vehicles, but at the Lang-gasse, in Mauer, the total width of the road between the houses is only 18 feet 4 inches (5·6 metres), including a footpath of 3 feet 11 inches (1·2 metre). There is, however, sufficient width left for public traffic to pass without interfering with the tramway. The least distance of the tramway from buildings, except in extreme cases, is 5 feet. In the open country the road averages a width of 29 feet 6 inches over all, and here also the tramway is kept to the one side. It is a single line, with passing-loops at the stations, the latter being timber structures of a simple character. The tracks at the passing-places are 10 feet 4 inches apart centre to centre, and the loops are 164 feet long. The rails are 4 inches high, with a base of $2\frac{3}{8}$ inches, and a head $1\frac{3}{8}$ inch broad, with tie-bars, resting on longitudinal timbers carried by cross-sleepers. The minimum radius of curves is 66 feet, and the steepest gradient 1 in 33. Two railways are crossed on the level.

The rolling-stock comprises ten locomotives and forty-three cars. The engines are completely boxed in. They are 15 feet 5 inches long, and 8 feet broad, with three coupled axles. The heaviest weigh when empty $17\frac{3}{4}$ tons. Under ordinary circumstances, a tram consists of only one car and the engine.

The cars are built on the American system, with a through gangway from end to end, a small portion being partitioned off for first-class passengers. The total length of the tramway is 6·43 miles (10·37 kilometres), and it takes fifty-six minutes to perform the whole journey, which includes stoppages of from half to one minute at the fifteen intermediate stations. The fares for the full distance are 9·28*d.* (40 kreuzers[1]) for first class, and 5·8*d.* (25 kreuzers) for second class, equal to a rate of 1·44*d.* and 0·90*d.* per mile respectively (3·85 and 2·42 kreuzers per kilometre). There are fifteen trams each way per day. The rate of speed averages 8·4 miles per hour. The least distance between any two stations is 330 yards, and the greatest 1,585 yards.

D. G.

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[1] The kreuzer is assumed as equal to 0·232 penny.

On the Electrical Furnace, and the Reduction of the Oxides of Boron, Silicon, Aluminium, and other Metals by Carbon.

By E. H. Cowles, A. H. Cowles, and C. F. Mabery.

(American Journal of Science, 1885, p. 308.)

The Authors have introduced a method of obtaining a continuous high temperature by introducing a material of high resistance to conductivity into the path of an electric current. After many trials, coarsely-pulverized carbon was selected, both for maintaining a variable resistance, and as a reducing agent for the oxides operated upon. When a mixture of carbon with the oxide to be reduced was made part of an electric circuit in a fire-clay retort, and subjected to the current of a powerful dynamo, not only was reduction effected, but so high a temperature was realized that the whole contents of the retort were completely fused; for instance, lumps of lime, sand, and corundum were melted and crystallized in well-defined forms on cooling.

The first experiments on a working scale were conducted with a current from two dynamos requiring 50 HP. The fire-brick walls of the furnace are protected by making a central core of a mixture of the ore and coarsely-powdered gas carbon, which is surrounded on the sides and bottom by fine charcoal; the current is led through the lesser resisting central core by carbon electrodes placed in contact with it at the ends of the furnace. The machines are protected from the variable resistance within the furnace by introducing a resistance-box containing a coil of German-silver wire in a tank of water in the main circuit, the quantity of current absorbed being indicated by a Brush ammeter on a shunt-circuit. The furnace is closed at the top with fire-brick slabs, well perforated for the escape of the gaseous products, and all the joints are luted air tight with fire-clay. A few minutes after starting the dynamo, a stream of carbonic oxide issues through the openings in the cover, and burns with a flame 18 inches long. Complete reduction is usually effected in about an hour.

The experiments hitherto made show that aluminium, silicon, boron, manganese, magnesium, sodium, and potassium can be easily reduced from their oxides. A large proportion of the charcoal also changes into graphite, but it is not as yet fully determined whether this is a result of fusion or of solution in the reduced metal. A large Brush machine, capable of utilizing 125 HP., or two and a half times larger than those hitherto constructed, is now being made for the Authors. A large water power of 1,200 HP. is also intended to be brought into use.

The products at present made are, aluminium-bronzes of various kinds, from a rich furnace-product obtained by adding copper to the ore; silicon bronze, aluminium silver, and alloys of aluminium with other metals. Boron bronze may also be produced by reducing boracic acid in contact with copper. Bronze with 10 per cent. of aluminium can be sold in quantity at prices based upon 20*s.* per lb. for the aluminium contained; the current rate for the lowest priced alloys of this kind being about 60*s.* per lb.

As regards the properties of the alloys made, the Authors find that the 10 per cent. aluminium-copper alloy (the original aluminium bronze) is of a fine golden colour, and has a tensile strength of 109,000 lbs. per square inch: the 5 per cent. alloy resembles 18-carat gold, with a tensile strength of 68,000 lbs. Copper with 2 to 3 per cent. aluminium is stronger than brass, discolours less rapidly, and may be usefully substituted for it. All these alloys cast well, and do not waste on re-melting, like zinc brasses. With 13 per cent. of aluminium, the alloy is hard and brittle, but still red; with 25 per cent. the colour is greyish black; with more than 25 per cent. the strength again increases.

Silicon in small proportions greatly increases the tensile strength of copper, but more than 5 per cent. makes the metal brittle. The effect is probably in part one of fluxing the contained oxide.

Boron seems to affect copper much in the same way as carbon does iron. When in small proportion, the metal has a tensile strength of 50,000 to 60,000 lbs. per square inch, without any sensible diminution of conductivity.

The alloy known as Hercules metal, consisting of copper and nickel, with a small proportion of aluminium, broke without elongation at 100,000 lbs. per square inch; another kind gave way at 110,000 lbs. with 33 per cent. elongation. These tests were made with castings as taken from the moulds. The strength of common brass is doubled by the addition of 2 to 3 per cent. of aluminium.

Alloys of iron and aluminium are easily obtained: one product contained 40 per cent. of the latter metal. In the furnace iron is not readily absorbed by reduced aluminium in the presence of copper; but in one experiment a mixture of 60 per cent. of old files, with 5 per cent. of nickel and 35 per cent. of a 10 per cent. aluminium-bronze, was melted, and gave a malleable product that stood a strain of 69,000 lbs.

Aluminium, when reduced without other metals being present, absorbs carbon, forming a metal analogous to that produced from iron ore in the blast furnace; such products contain 60 to 70 per cent. of aluminium. Silicon is also readily taken up when the ores are siliceous, and compounds containing 10 to 15 per cent. of this metallized have been produced in considerable quantity. These are at present under examination.

H. B.

The present Value of American Steel-Castings.

By A. V. Abbott.

(Transactions of the American Institute of Mining Engineers, Sept. 1885.)

The Author, having had occasion to use steel-castings to a large extent in the manufacture of testing-machinery and large scales for Messrs. Fairbanks and Co., and, finding that on several occasions such castings failed under stresses far below what were claimed as their ultimate strength, has instituted a series of experiments to determine the constructive value of the steel castings now obtainable in the United States. Two patterns were sent to each steel foundry, one being a straight bar, 30 inches long and 2 inches square, and the other a bar 1½ inch square for 6 inches at either end, but turned cylindrical for a length of 12 inches in the centre. The first was intended to supply transverse and compression test-pieces, and the second those for tension. Two castings were ordered from each pattern, to allow for checking doubtful results. The patterns were sent to ten different steel-works, in Pennsylvania, New York, and Ohio, but only seven supplied the required castings. The tests were made with the steel as removed from the sand, without removing the skin, which is generally supposed to be an element of importance in castings. The Author, however, considers this to be a mistake in the case of steel, as the outer layer appears to be less ductile than the interior, and yields first under strain, and that therefore higher results would have been obtained if the outer surfaces of the test-pieces had been removed.

The sizes of the test-pieces were as follows :—

For tension . . .	1½ inch diameter, 10 inches long between gauge points.		
,, compression .	2 inches square, 2¼ feet long.		
,, transverse stress	2 ,,	2 ,,	loaded in the centre.

No.	I.	II.	III.	IV.	V.	VI.	VII.
	Lbs. per Square Inch.	Lbs. per Square Inch.	Per cent.	Lbs. per Square Inch.	Lbs. per Square Inch.	Lbs. per Square Inch.	Inch.
1	28,923	43,631	6·0	35,011	6,000	8,000	0·72
2	25,610	36,880	0·4	31,250	2,500	3,870	0·45
2	25,650	42,280	0·8	..	2,600	3,950	0·52
3	23,116	44,221	6·0	24,350	3,000	4,181	0·88
4	21,395	38,301	10·0	26,750	3,950	5,720	0·15
6	31,180	46,156	2·0	29,703	4,000	6,259	0·60
7	35,840	56,724	10·0	36,110	4,000	5,480	0·20
9	35,445	52,330	9·0	27,027	4,000	6,810	0·21
9A	38,217	63,058	29·9	..	..	..	..
9A	38,217	62,816	20·5	..	..	..	..
10	17,522	32,151	8·0	20,408	25,000	4,010	0·35

I. Elastic limit in tension; II. Ultimate tensile limit; III. Elongation in tension; IV. Elastic limit in compression; V. Elastic transverse limit; VI. Ultimate transverse limit; VII. Transverse deflection.

Nos. 9A were test pieces of steel submitted for use in shipbuilding for the U.S. Navy. They were turned from end to end, and reduced in the centre for a length of 2 inches to a diameter of 1 inch.

Nos. 3, 7 and 10 showed perfectly sound fractures; all the others were defective, either from blowholes or soft centres. Nos. 6 and 9 were seriously affected by blowholes.

Nos. 2 and 3 cost 6*d.*, No. 4, 5½*d.*, and Nos. 1, 6, 7, 9 and 10, 5*d.* per lb.

The tests were made with Messrs. Fairbanks' testing-machine at New York.

The average results of the tension tests were; elastic limit, 29,400 lbs. per square inch; modulus of elasticity, 24,430,000 lbs., and elongation 9 per cent. Except in the latter factor, which is low, these results are comparable with those obtainable from good iron, but in two instances a very decidedly superior material was indicated. In comparison with cast-iron, it is at the least calculation three times as efficient for construction.

The character indicated by the transverse tests are similar, except that the lack of ductility tells badly against the material, and causes rupture sooner under the form of strain than would have been anticipated from the tension result.

In compression, the rigidity of the material compares more favourably with wrought metal, especially when lateral or bending strain is avoided; but between steel-castings and cast-iron in compression the comparison is less favourable to the former, as in this form of stress the materials closely resemble each other.

The cost of steel-castings from the mould is assumed as about 30 per cent. higher than that of iron- and steel-forgings, but for shapes that are at all complicated the former are the more economical, as there is little or no waste metal, nor is any expensive amount of machine work required for shaping, as is the case with rough forgings. Therefore, for parts of machinery that are subjected to anything but direct compression, steel-castings as at present made may be considered as successful rivals to forged work. When, however, compression is alone to be resisted, cast-iron will for a long time to come hold its own; for, subjected to this stress, the steel is but little superior to the iron casting, and the great difference in price will continue to make the latter the successful competitor.

H. B.

On the Influence of Temperature on the Strength of Iron and Steel. By B. Papkoff.

(Gorny Jurnal [Russian Mining Journal], St. Petersburg, August 1885, p. 341.)

It is well known that low temperatures have a considerable influence on the strength of metals, and this fact has great importance in reference to the strength of railway and other structures in cold northern countries. The Author intended to carry out an extensive series of experiments on the influence of cold on the strength of iron and steel in various forms and under various kinds of strains, but circumstances obliged him to abandon his intentions after a very few tests had been made; he thinks, however, that the results he was able to obtain are worth being made known, because they seem to point to conclusions totally opposed to those generally received.

All the specimens tested were taken from soft steel and iron plates, three samples being cut from each plate. One sample of each group was tested at the ordinary, and two at low temperature, with the results given in the following Table:

Materials Tested.	Temperature. Fahrenheit.	Ultimate Strength in Tons per Square Inch.	Percentage of Elongation in 8 Inches.	Time of Exposure to Cold.
I. Three strips of dephosphorized steel plate . .	+63½	22·73	25·9	..
	− 2	23·75	25·5	1 hour.
	− 2	26·10	27·6	3½ hours.
II. Three strips cut out of iron plate, length with the grain . . .	+63½	17·91	10·1	..
	− 2	20·00	14·0	2 hours.
	− 2	19·30	16·3	2½ hours.

It will be observed that both the ultimate strength and the percentage of elongation increases very sensibly with the decrease of temperature, and the Author remarks that such a result was to be expected because the contraction caused by cooling has the effect of bringing the particles of matter closer together, and consequently of intensifying the force of cohesion; but he also observes that a law which may be found general for strains gradually imposed, may not apply at all when they assume the nature of shock or of impact.

W. A.

Calorimetrical Studies on the Effects of Sudden Cooling and Hardening on Molten Steel. By — Osmond.

(Beiblätter zu Wiedemann's Annalen der Physik und Chemie, 1885, p. 569.)

The Author dissolved different steels in solutions of copper chloride and ammonium chloride, and determined the relative production of reaction-heat.

	Percentage of Carbon.	Made soft.	Hardened.	Suddenly cooled.
Soft steel	0·1700	1·000	1·045	..
Medium ,, ...	0·5400	1·000	1·045	1·052
Hard ,, ...	1·1700	1·000	1·065	1·084
White cast iron from Sweden	4·1000	1·000	..	1·050

The hardening being either by sudden cooling, or by otherwise treating, increases the reaction-heat. From this the Author concludes that there are two modifications of iron, the one crystalline is formed by slow cooling, the other amorphous by quick cooling,

&c., but these particular modifications only occur when carbon is present.

Copper similarly treated does not show any difference in reaction-heat in either the hard or soft modifications.

C. A.

Shaft-Sinking with Temporary Iron Lining.

(Zeitschrift für berg- hütten- und salinenwesens, vol. xxxiii., 1885, p. 223.)

At the Gerhard colliery, Saarbrücken, the shaft Victoria No. II., commenced in June 1881, and completed to the depth of 283 metres in August 1884, has been lined with brickwork, iron rings being used to support the ground during the sinking instead of timber.

The pit of 5·40 metres diameter was sunk by dynamite and electric ignition. The iron rings, 5 metres inside diameter, of ⊔ section, 215 × 87 × 14 millimetres, put together in four segments. The uppermost one of each series of four or five rings was supported upon pine-wood bearers, bedded at the ends in the rock, and the lower ones were suspended from it and each other by iron hangers, 2 metres long. The clear space between each ring was therefore 1·57 metre, this was filled up by wooden struts. By a series of temporary bearers of square timber attached to the rings, the shaft was divided into drawing and footway segments, the latter being closely boarded off from the drawing compartments in order to facilitate ventilation.

The permanent walling was commenced at a depth of 25 metres by contracting the diameter of the pit 0·5 metre, and laying the first crib, a cast-iron ring, 40 millimetres high, 4·40 metres inside, and 5·40 metres outside diameter. This is put together in eight segments, and upon it the shaft wall is built in brick and cement, 2 bricks (0·5 metre) thick, and 4·40 metres interior diameter. As the walling advanced the provisional lining was removed, and the permanent buntons for the guides were fixed. There are ⊔ iron girders about the same length as the inside diameter of the pit, placed across it, and built into the wall at either end. They were protected against bending by short perpendicular timber struts projecting from the wall behind them. This part of the work was done in three eight-hour shifts, the building going on by two shifts, while the third was devoted to the removal of the provisional lining.

As soon as the first length of walling was finished up to the surface, the sinking was resumed in the same manner, a sufficient bed for the crib being left. The second crib was fixed at 50 metres, and the second length of ground was walled up in the same way as the first.

At this point a rise was commenced from the lower workings, starting from a drift 21 metres above the bottom level, in order not to interfere with the construction of the works at the pit-eye.

This rise of 3 metres in diameter was divided by cross timbers into three unequal parts, the two smaller ones serving for ladders and air-ways, while the third, of about three-quarters of the whole area, was filled with the shaft broken from above.

In the third section the influx of water from the rock diminished, so that the thickness of the wall was diminished to 1½ brick below 50 metres. The same ironwork, with somewhat lighter plank linings, was used again, and the whole depth of 283 metres was finished with the use of only sixty-six rings, so that each ring was used on an average 2·4 times.

At 132 metres the third crib was laid, and at 146·5 metres the sinking was connected with the rise from the below. The latter was then cut out to the full width of 5·20 metres down to 250 metres, when the fourth crib was laid, and while this was being walled up the remaining ground above the bottom level was cut through from below.

The pit-eye is an octagonal chamber about 9 metres across, with a domed roof, which, in order to give headroom for handling a double-decked cage, is carried up to nearly the same height. The junction of the dome with the pit wall is effected by a wedge-shaped ring of cast-iron, put together in eight segments, weighing about a ton each. The cages are guided through the domed space by a free standing frame of wrought-iron. The sections of the different parts of the sinking, with the arrangement of the dividing partitions, are illustrated in the original.

H. B.

Stone Cribs for Watertight-Shaft Masonry.

(Zeitschrift fur berg- hütten- und salinenwesen, xxxiii., 1885, p. 224.)

At a deep sinking at Osterwald, sandstone rings, put together in ten segments, have been used instead of cast-iron cribs. Two thicknesses of rings are laid one above the other, the upper one having a small overhang inside. This gives room for an internal wooden ring, by which the lower ring is provisionally strengthened. When the next length of walling below is finished, the wooden ring is removed, and the hollow space is filled up with a ring cast in cement. The joints of the cribs are wedged with wood, ten thousand strips 1 centimetre thick of fir and two thousand of oak being required for each ring. A narrow semicircular gutter cut in the inner projecting edge of the upper ring, leads the water that collects during the wedging into the shaft. The walling is built in sandstone, and the space behind it is filled up with concrete. A cast-iron tube with a valve opening upwards is fixed in the concrete behind the crib, to allow water collecting behind it to pass upwards, and so relieve the pressure. These cribs are considerably cheaper than cast-iron ones.

H. B.

Statistics of the Breakage of Pit-Ropes.

(Dingler's Polytechnische Journal, vol. cclviii., 1885, p. 140.)

In the Dortmund district of the Westphalian coal-fields, an account has been kept of the wear of the ropes of the different collieries since 1871, from which the following Table has been compiled for the thirteen years, 1872–1884.

During this period two thousand six hundred and sixty-nine ropes have been laid aside as worn out, and of these one hundred and eighty-six, or 7·85 per cent., broke suddenly while at work. Classified according to shape and material, the results were—

	Total.	Broken suddenly.	Per cent.
Flat cast-steel rope	207	19	9·18
,, soft iron ,, ...	147	19	12·9
,, aloe fibre ,, ...	74	6	8·11
,, hemp ,, ...	8	0	0
Round cast-steel ,, ...	1,118	42	3·75
,, soft iron ,, ...	815	100	12·3

The number of broken ropes in the different years were—

	Total.	Broken suddenly.	Per cent.
1872	114	22	19·3
1873	198	22	14·1
1874	156	19	9·64
1875	226	19	8·40
1876	227	15	6·91
1877	178	16	8·98
1878	202	19	9·40
1879	172	9	5·32
1880	170	8	4·70
1881	165	8	4·85
1882	194	15	7·73
1883	187	8	4·27
1884	190	6	3·16

Whence it appears that, with a few interruptions, the proportion of failures of pit ropes by sudden breakage, to those regularly worn out, has continuously diminished during the thirteen years that the operations have been carried out.

H. B.

Metallic Guides for Cages in Shafts: Briart's system.

By F. Bollaert, Engineer at the Bascoup Mines.

(Société de l'Industrie Minérale, Comptes-rendus Mensuels, July 1885, p. 105.)

In the St. Catherine pit at Bascoup, 362 yards deep and 14 feet in diameter, which is bricked, but has no tubbing, the girders, which are of I section, weighing 39 lbs. per foot, or 595 lbs. each, are 15 feet 3 inches long, and fixed with oak wedges into niches in the walling. They are notched to receive the rails, which are of Vignoles section, weighing 73 lbs. to the yard. They are 29 feet 4 inches long, with an end-clearance of 2 inches, to allow for expansion. As the girders are spaced 14 feet 9 inches apart vertically, there is one at each rail-joint, and one intermediate. The fixing is done from the top downwards, by means of a movable platform, which can be suspended either from the winding-rope or from the girders already fixed, and the rails are carefully plumbed and gauged. Three shifts of men were employed, and the work was completed in twenty-two days, or at the rate of 16 yards per day. This is exclusive of the length of 60 feet from the surface to the pulley-wheels, which was specially fixed by another gang of men. The cages are guided by slippers bolted to them, which clasp the bulb-heads of the rails.

This arrangement answered very well for the comparatively small cages used at Bascoup, weighing only 4 tons when loaded; but with the heavier cages at Lens, carrying eight tubs in two tiers, with a total weight of from 7½ to 8 tons, often unequally distributed, and liable to be canted out of plumb, a different arrangement was adopted. In this case the girders were spaced only 4 feet 11 inches apart vertically, but were much lighter, weighing only 14 lbs. per foot, or 220 lbs. each. Only one pair of clamps were used at each girder for each pair of rails back to back (instead of two pairs as at Bascoup), except at the joints, where two pairs were used. The rails were 29 feet 5½ inches long, with a clearance of ¾ inch, and rested on the girders by means of a small block riveted to the bottom end of each rail, leaving them free to expand in an upward direction only.

In No. 3 pit at Lens, with a total depth of 394 yards, 72 yards were lined with wood tubbing, to which the girders were fixed by means of wooden brackets nailed on. In the other part, cast-iron sockets were built into the masonry, into which the girders were wedged. The fixing was completed at the rate of 12 yards per day, the use of safety-platforms and the greater number of cross-girders delaying the work as compared with that at Bascoup.

The total net cost, and the cost per yard, may be seen from the following Table :—

Materials.	Bascoup, 362 yards.		Lens, 394 yards.	
	Total.	Per yard.	Total.	Per yard.
	£.	s. d.	£.	s. d.
Cross-girders	135	7 0	226	11 6
Rails	306	15 11	225	11 4
Clips, bolts, distance-blocks, &c. . . .	212	11 0	148	7 6
Wedges for fixing girders	19	1 0	5	0 3
Cast-iron sockets for girders	..	..	84	4 2
Wooden chocks and brackets	..	..	3	0 2
Wear and tear of tools, &c.	59	3 1	61	3 0
Total materials	731	38 0	752	37 11
Labour (pitmen, bricklayers, labourers, enginemen, &c.)	133	6 11	167	8 5
Total materials and labour . .	864	44 11	919	46 4

But it must be noted that the conditions were dissimilar in the two cases, and also that certain extras, such as the guides above the surface at Bascoup, and other items at Lens, are included in the above figures. Making due allowance for these, the corrected figures should be as follows :—

——	Bascoup.		Lens.	
	s.	*d.*	*s.*	*d.*
Material	36	11	31	0
Labour	5	3	9	2
Total per yard	42	2	40	2

The above figures, compared with the cost of wooden guides, which, moreover, have a much shorter life, may be considered as very satisfactory.

W. S. H.

Prevention of Overwinding. By — Baure.

(Bulletin de la Société de l'Industrie Minérale, vol. xiv., 1885, p. 77.)

With the high speeds of winding now employed, the adoption of a lofty head-gear, with a bell and indicator in the engine-house, are not sufficient precautions against over-winding, while neither pawls, to prevent the fall of the cage down the shaft, nor safety-hooks to release the rope, the loose end of which, flying at full speed into the engine-house, is liable to do serious damage, meet all the requirements of the case.

A common arrangement, and one which is very satisfactory when the upper part of the head-gear is sufficiently strongly framed, consists in bringing the guide-rods gradually nearer together at the top, so that the cage wedges itself, is brought to rest without shock, and remains suspended, even if the rope breaks. Safety-pawls may be added as an extra precaution, but are not necessary, as the cage cannot fall until the guides are slacked back.

For bringing the engine to rest, there is an arrangement which has long been in use in Belgium, consisting of an endless screw driven from the drum-shaft by bevel-wheels, and carrying two nuts or runners which, at either extremity of their travel, release a catch, and thereby close the throttle-valve in the main steam-pipe, put on the brake, and open the mud-cocks on the cylinders.

Another arrangement, based on the invention of Mr. Goutteaux, of Gilly, in which effects similar to the above are produced by a lever actuated by the cage itself, as soon as it has passed the prescribed limit, has been adopted at the Saint Claude pit of the

Comberigol concession (Loire), with such success as to justify its repetition in other cases. A rocking-shaft, fixed near the top of the head-gear, carries two levers, which are encountered by one or other of the cages when lifted too high, a balance-arm, and another lever connected by a long rod with a second rocking-shaft in the engine-house, keyed on which are the requisite levers for closing the throttle-valve, admitting steam to the brake-cylinder, and opening the cylinder cocks. There is also another double-ended lever keyed on the first rocking-shaft, with hand-chains hanging from each end, by which the apparatus can be replaced in its normal position, or, in case of emergency, can be brought into instantaneous action, so as to stop the engine at once.

An objectionable feature in this apparatus, as at first adopted, was that it temporarily deprived the engineman of the control of his engine, which he could neither start nor reverse. To obviate this rather serious defect, the connection with the rocking-shaft in the engine-house, instead of being a positive one, was, in the later examples, made by means of a pin engaging in a gab at the end of the long connecting-rod, which could be tripped by a foot-lever, thus leaving the engine entirely independent.

The Paper is illustrated by detailed drawings, showing various modifications of the apparatus to suit different cases, and several instances of its successful application are quoted.

W. S. H.

A Speed-Indicator for Winding-Engines.

(Zeitschrift für berg- hütten- und salinenwesen, vol. xxxiii., p. 237.)

At the No. 1 Camphausen pit of the Dudweiler Colliery, Saarbrücken, a speed-indicator, designed and constructed by Master Mechanic Gerhard, has been attached to the winding-engines. This consists of a centrifugal governor inclosed in a cast-iron case, which is driven by a pair of bevel wheels, receiving motion from a strap and pulley on the drum-shaft of the engine. The spherical weights of the governor are attached to the vertical arms of a pair of bent levers, which are connected with a sliding collar on a rod above, in such a manner that it is at its highest point when the engine is stopped, but is drawn down by the divergence of the arms when they are set in motion. This motion is transferred by a system of multiplying levers to a large index pointer, which moves over a divided arc above the regulator-box. The regulator works against a spiral spring which tends to keep the sliding collar in the position of rest; but as the elasticity of the spring does not increase in the same proportion as the centrifugal force of the rotating mass, it is supplemented by a flat bar spring, bent nearly to a semicircle, which is attached to the arms by its ends. The tendency of this is to force the arms apart when at rest, and to bring them together as soon as a certain speed has been attained. In this way a nearly uniform deflection of the pointer is attained for similar increments of speed. The scale is a circular arc of about 15°, and indicates speeds from 0 to 20 metres per second.

In addition to the outside pointer a large indicator barrel is placed in a glass-case above the top of the regulator, upon which the movement of the regulator is traced by a pencil. This receives a slow movement of rotation from a driving-clock, and makes one revolution in twelve or fourteen hours, according to the length of time that the engine is employed during the day. The diagram thus obtained gives a complete record of the work done by the engine, as well as of the length of time consumed in stoppages.

H. B.

Underground Steam-Engines at the Bézenet Colliery (Allier).

By — BAURE.

(Bulletin de la Société de l'Industrie Minérale, vol. xiv., 1885, p. 279.)

The use of fixed engines underground is often found advantageous for many purposes, such as underground haulage, sinking trial-shafts, driving ventilating- or pumping-apparatus, or working to the dip, and thereby saving the expense of sinking fresh shafts and driving cross drifts. Such engines are usually driven by compressed air, water-pressure, electricity, or steam. Gas-engines, and driving by exhausted air, are not found suitable for underground purposes. In some collieries ropes worked by engines above ground, and conducted down the shaft and along the main roads, are employed, but where the roads are irregularly laid out and timbering operations are of frequent recurrence, ropes are objectionable and unsatisfactory, besides encumbering the winding shafts.

The employment of compressed air has the advantage of improving the ventilation and lowering the temperature, but the heavy expense of first outlay and subsequent maintenance of the plant, including engine, boilers, air-compressor, and reservoir on the surface, in addition to the underground engine, and the loss of effective power, amounting from various causes to some 75 per cent., are serious objections to its use except under special circumstances.

There is much to be said in favour of water-pressure engines in cases where the power of the pumping engines is in excess of the demands made on them, and where the exhaust-water can be easily disposed of, and many examples of its advantageous employment can be cited, but these circumstances unfortunately did not exist at the Bézenet colliery.

Where the power to be transmitted is not more than from 20 to 30 HP., and the distance does not exceed from 1 to 2 kilometres (⅝ mile to 1¼ mile), electricity has frequently been employed with very satisfactory results, but the losses in transmission are considerable, and vary so much in different instances, and even in the same installation when the power to be transmitted is not uniform, that it is impossible to make any accurate comparison as to useful effect with air or water under pressure. According to the experiments of Mr. Charousset at La Peronnière, and of Mr. Graillot at the Blanzy collieries, the useful effect would seem to be about equal to that obtained from compressed air, and the adoption of one or the other must be determined by special circumstances. Currents at high tension are exceedingly dangerous underground, giving severe or fatal shocks if accidentally touched, and although the conductors take up less room than air or steam pipes, and are more readily adapted to irregular and winding passages, and do not raise the temperature, yet the dynamo-electric machines underground are with difficulty protected against moisture and dust, and require more care than the less delicate compressed-air motors, with which moreover the workmen are more familiar.

Steam-engines and boilers, both placed underground, offer the most simple and economical arrangement, and are very frequently adopted in England, where a spacious boiler-house can generally be excavated out of the solid rock. But in the Belgian and French collieries these favourable circumstances rarely exist, and heavy walling or timbering is required, while the ventilation is seldom good enough to sweep out the smoke and steam. To this must be added the risk of explosion and fire, the inconvenience of the hot gases in the winding pit, and the deterioration of the wooden guide-rods and other fittings; while the water underground is usually too bad to be used in the boilers, and the supply has to be brought from the surface, and the necessarily confined space renders inspection, repairs and replacement very difficult.

The foregoing considerations led to the adoption, at Bézenet, of steam engines underground supplied from boilers on the surface. Many examples of this arrangement exist in England, Belgium, France, and Prussia, and the objections usually made to it are: loss by condensation in long ranges of pipes, and consequent waste of coal; rapid destruction of the lagging of the pipes, involving expensive and difficult replacement; failure of the expansion-joints to allow free expansion and contraction after they have been at work a short time; and danger to the engineman from sudden escapes of steam in the engine-room.

After two experiments on a comparatively small scale, in which steam was conveyed from the surface to a distance of 113 yards and 223 yards respectively, a third engine was erected in the Sainte Barbe pit, at a distance of 125 yards from the shaft, the total length of pipes being 450 yards. The pipes were of cast-iron, with a bore of 0·8 metre (3·15 inches), and, to avoid condensation as far as possible, were clothed with a triple lagging, the outer lagging being of pine-wood staves, hooped with screw-bands, and the middle layer being composed of old strands of worn-out hemp or aloes rope. For the innermost layer, after trying felt of 1 centimetre ($\frac{3}{8}$ inch) thick, which, though it proved a good non-conductor, soon crumbled to dust, and a mixture of clay and old chopped hemp, a simple layer of cement from 12 to 15 millimetres (0·47 inch to 0·59 inch) was adopted as the most convenient and simple coating.

Previous experience having shown that expansion-joints with stuffing-boxes soon get set fast with rust and deposit, copper pipes, bent so as to spring and allow for expansion and contraction, were inserted at suitable distances.

To intercept the condensed water, two wrought-iron receivers were inserted in the line of pipes; one near the engine, where it could be easily drawn off by a blow-out cock worked by hand; the other near the bottom of the shaft, 257 metres (281 yards) from the boilers. An automatic blow-out apparatus was originally fixed to the latter, but proving unsatisfactory, was subsequently removed.

The exhaust-pipe, of cast-iron, 0·12 metre (4·72 inches) bore, was led away to a small shaft communicating with a lodge of water 150 metres (161 yards) long, and from 2·50 metres to 3 metres (8 feet to 10 feet) wide, forming a condensing pond large enough for a much more powerful engine. Experiments made with a range of pipes 60 metres (66 yards) long, immersed in a trench of water, forming a rough surface condenser, were unsatisfactory, as the vertical distortion of the pipes, in spite of all care in fixing, hindered the free escape of the condensed water. It was therefore decided to fix the exhaust-pipe to the side-wall of the drift, with a regular fall from the roof to the floor and into the reservoir. A very short length being submerged, the condensed water easily escaped, and the pipe was always clear. Owing to the vacuum thus formed, and the relatively large size of the exhaust-pipe, the back-pressure was insignificant. The steam-pressure was sensibly the same at the cylinder as at the boiler, the loss by condensation being compensated for by the weight of the vertical column of steam 186 metres (610 feet) high.

By experiments made on the 24th May, 1879, the average temperature of the gallery was found to be only 3° Centigrade (5½° Fahrenheit) above that at the pit mouth, and the engine-man was exposed to no higher temperature than 26° to 28° Centigrade (79° to 82° Fahrenheit).

The condensation per hour in the steam-pipes amounted to 1·18 kilogram of water per square metre (0·24 lb. per square foot) of pipe-surface above ground and in the shaft, and to 0·78 kilogram per square metre (0·16 lb. per square foot) in the main road, giving a mean of 1·03 per square metre (0·21 per square foot), allowing for the relative lengths of each range of pipe; and these figures are easily accounted for when it is remembered that in the St. Barbe shaft the pipes are constantly wet for the greater part of their length. Probably 10 per cent. should be deducted from the above figures, as being the result of priming, not of actual condensation; and as the engine was but a small one, developing only some 12 HP. effective, smaller pipes might have been used, and the loss by condensation thereby reduced by 20 per cent.

As the fourth engine was fixed at a greater depth than that last described, the lagging of the steam-pipes, with a vertical length of 300 metres (328 yards) in the wet shaft, demanded especial care. Magnat's non-conducting composition was finally adopted, covered with an outer cylindrical casing of thin sheet iron, made in halves bolted together, in length to correspond with the distance between the pipe flanges, and of a conical shape at each end, so as to allow free access to the flange bolts. The loss by condensation was found to be 1·79 kilogram per square metre (0·37 lb. per square foot) of pipe-surface per hour, or 17·30 per cent.; but as in this case also the diameter of the steam-pipes might have been reduced, 13 per cent. of this loss might have been saved.

To sum up :—When the ventilation is good, and there is no fear of unduly raising the temperature underground, and when the exhaust steam can be conveniently got rid of, steam conveyed from the surface is a more economical method of transmitting power than either compressed air or electricity, and the loss of effective power need not exceed 15 to 20 per cent. in the wettest shafts, or a much lower proportion in dry shafts and main roads.

The Paper contains detailed results of experiments with various non-conducting coverings, and is illustrated by plates showing the general arrangements adopted.

W. S. H.

Animal- and Locomotive-Power in Pennsylvania Mines.

By A. W. Sheafer.

(Proceedings of the Engineers' Club of Philadelphia, August 1885, p. 150.)

The relative cost for haulage in the anthracite mines of Pennsylvania, by mules and by locomotives, has been ascertained from the results of the work of the year 1822, in Kalmia colliery, Schuylkill county. The thickness of the bed of coal varies from 6 feet to 10 feet, and 201,000 tons were taken out in that year. The main gangway is about three miles long, principally straight line, and the round trip is 6 miles. Three locomotives—Baldwin inside connected—are in constant use. The trip is made by a locomotive in one hour and forty minutes, with a steam-pressure of 105 lbs. per square inch, hauling a train of from eighteen to twenty-six cars, each of 2½ tons capacity; and a total of one-hundred and thirty-three cars per day. The coal is consumed as fuel at the rate of 1,097 lbs. per day, costing 5*s.* 7½*d.* Oil, waste, packing, &c., cost 2*s.* 11*d.*; repairs and renewals are estimated at 2*s.* 6*d.*; wages to engineer and switch-boy, &c., 13*s.* 10½*d.*; making a total cost for locomotive power, of £1 4*s.* 10*d.* per day. For animal service, sixteen mules are required for the same duty, costing for feed, shoeing, repairs to harness, depreciation and renewal, 3*s.* 5½*d.* per mule, or, for 16 mules, £2 15*s.* 4*d.* The wages of drivers, at the rate of one for 6 mules, at 7*s.* 1*d.*, amount to 18*s.* 10½*d.*; oil and cotton, 1*s.* 8*d.*; in all for horse-power, £3 15*s.* 10½*d.* per day, against £1 4*s.* 10*d.* for locomotive power.

D. K. C.

Steam-Boiler adapted for Shipment. By P. CHEVILLARD.

(Revue Industrielle, 1885, p. 373.)

Mr. V. Cadiat has designed a water-tube boiler suitable for shipment abroad, combining great power in small bulk, lightness, solidity, durability, and facility of access for repair. A group of sixty water-tubes is placed at an angle of 45°, opening at the upper ends into a spherical receiver, and at the lower ends into a spherical segment; the segment, the tubes, and nearly half of the receiver being filled with water. The fireplace is near the tubes, and the flames strike directly upon them, and traverse them before escaping by the chimney at the back. A priming-separator is erected in the upper part of the steam space in the sphere, in which the steam-pipe is coiled into a helix of three revolutions, through which the steam passes on its way out of the boiler. The helix is perforated by small holes at the outer side, adapted with inclined planes, through which the water, mixed with and drawn into the pipe by the steam, is rejected by centrifugal action. The furnace is of sheet-iron, lined with firebrick.

A boiler of this class was tested at the Toulon Arsenal, on a canoe. It had 60 square feet of heating surface, and 2 square feet of firegrate. It was stamped for 5 atmospheres of pressure, and weighed, with fuel and water, 2,123 lbs. Steam was got up in twenty minutes, and combustion proceeded at the rate of 106 lbs. of briquettes per hour, 748 lbs. of water being evaporated per hour, or 7 lbs. per lb. of fuel. From 8 to 10 per cent. of water was carried over as priming.

D. K. C.

Progress in the Utilization of Natural Powers—Yagn Motor.

By N. DE TEDESCO.

(Electricien, vol. ix., 1885, p. 610.)

In spite of the incontestable superiority of electric light, it is certain that it cannot come into general use until a motor is provided, by which such of nature's forces as are widespread and almost constant in their action can be utilized.

These forces, however, so freely granted, are restricted in their application; atmospheric electricity is still in the clouds, solar heat is sufficient only in regions far removed, as a rule, from industrial centres; the wind is capricious, and the tides require installations of too costly a character.

The potential energy of rivers in their flow, which in France alone would yield at least 16 million horse-power, is best adapted for the purpose; but the hydraulic motors, efficient as they are, require a fall of water, obtainable in few places, and these generally remote from manufacturing districts.

A motor for utilizing the flow of an ordinary stream, extremely simple and effective, has been proposed by Mr. Yagn.[1]

A machine of this description has been erected by Mr. Fevrot on the Rhone to furnish power to the weavers of Croix Rouge, at a distance of 3 kilometres (nearly 2 miles), the transmission being effected by the medium of compressed-air.

The parachutes are 1·2 metre (47 inches) in diameter, the up-river drum is 4 metres (157 inches) in diameter, and rotates six times in a minute, or at a surface velocity half of that of the river current which is 2·5 metres per second (5·6 miles per hour); the power developed is 10 to 12 HP. when six parachutes are in action, the pressure on each of which is then 110 kilograms (242 lbs.).

Mr. Fevrot furnishes the whole installation of hydromotor, air-compressor, tubing, and compressed-air motor, and charges each subscriber only 0·2 franc (say 2*d.*) per day. Calculating at the rate of $\frac{1}{9}$ HP. for twelve hours, the cost per HP. per hour would be only 0·15 franc (1½*d.*), while a small steam-motor would certainly cost three times as much.

This result is certainly remarkable if these figures can be assumed as remunerative; and it would prove that this motor promises an economical and simple machine for the development of the transmission of power, especially as it must be remembered that it is still in its provisional form.

Without any interference with the navigation of the rivers, it is probable that 1 HP. per metre of stream-length could be thus developed, and that fourteen thousand workmen could be supplied with power at their homes for each mile of the stream. Or this power might be used to generate the current for incandescent lighting during the night, and for charging accumulators when not thus occupied.

F. J.

Cadiat's Brake-Dynamometer.

(Portefeuille Économique des Machines, 3rd series, vol. x., 1885, p. 175.)

This article describes a dynamometer, invented by Mr. M. V. Cadiat, which is in a certain sense automatic, in that the necessary resistance is adjusted by the brake itself, although it is not automatic in the sense of being self-recording, but requires a series of observations to be made at any particular time when the power absorbed is being measured. In general form the dynamometer resembles the well-known instrument of Prony; that is to say, a flexible metal band, to which are fastened a number of blocks of wood, is made to encircle a rotating pulley driven by the prime-mover whose power is to be measured, the friction of the band being regulated by a hand-wheel and screw, by which the required pressure between the pulley and brake-blocks is obtained. Here, however, the resemblance ceases; the novel feature consisting in the substitution of a rope which passes round the outside of the metal band, for the usual horizontal lever. To this rope is attached not merely the ordinary scale-pan, but an arrangement resembling a flexible ladder, the rounds or bars of which are known weights. Thus the variations in friction at the surface of the brake, which give so much trouble in the Prony form, where the lever is continually coming against either the upper or lower stop, in this case merely raise or lower the ladder, taking thereby a greater or less number of weights on the rope. Thus, as the rope passes round the circumference of the brake-band, and the effective radius is always constant, the number of weights raised is a direct measure of the resistance of the brake; and, knowing the velocity of the pulley, a single glance enables the power to be determined.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxx., p. 403.

The article, amongst other proofs of the satisfactory action of this dynamometer, which has been used for several years by Messrs. Ed. Mourraille and Co., of Toulon, refers to five hours' trial of an engine of 95 HP.

H. S. H. S.

On a New Form of Integraph.

By D. Napoli and B. Abdank Abakanowicz.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, p. 592.)

This instrument is for the purpose of tracing the integral curve ($Y = \int f(x)\,dx + c$), any curve of the form ($y = \phi(x)$) being given. Other instruments for a similar purpose have been described by the Authors, but this one has the two following advantages:—

(1) Absence of slipping between the surfaces of the parts in contact, which change their relative position by rolling on each other. (2) The actual dimensions of the parts do not affect the result as in former integrators, the direction in which certain parts are caused to move being the only important point where accuracy of adjustment is required.

The whole construction is made in a very careful manner, the details being specially attended to, and the curves, which are made by a drawing pen, are extremely well defined, both at their commencement and at their finish, no appreciable play in the fitting parts being allowed.

The following is a brief description of the instrument and its mode of operation:—Suppose the axes of co-ordinates of the given curve to be chosen; then parallel to one axis (the axis of x) an iron straight-edge having a groove in its upper surface is placed. In this groove run two rollers, which thereby guide across the paper a movable frame, consisting of two parallel straight-edges, at right angles to the first axis, and therefore parallel to the second axis (the axis of y). This movable frame carries two small trucks, the wheels of which roll upon, and are guided by, the parallel bars. To one of the trucks is attached a pointer or style, which is passed round the given curve, and this causes the simultaneous movement of the other truck carrying the pen which draws the integral curve. The principle upon which this is effected is the same as that adopted in the cart integrator of Mr. C. V. Boys,[1] and need not be here explained, but the method of obtaining the necessary parallelism between the plane of the cart-wheels carrying the drawing pen, and the tangent bar connected with the tracing style, appears to solve a difficult problem in a simple manner, but in a way not to be easily explained without the aid of a diagram.

H. S. H. S.

Determination of the Ohm.

By Messrs. MASCART, F. DE NERVILLE, and R. BENOIT.

(Annales de Chimie et de Physique, 6th series, vol. vi., 1885, pp. 5–86.)

The Authors describe a series of experiments made by them to determine the dimensions of a column of mercury at 0° Centigrade, which represents the unit of electrical resistance.

1st. *Weber's method.*—The absolute resistance of a conductor-circuit is measured by the quantity of induced electricity which traverses it when it is moved in the earth's magnetic field. For this purpose, insulated copper wire is wound on a mahogany frame or bobbin, free to move round a vertical axis; the plane of the circuit being at first at right angles to the magnetic meridian, it is suddenly turned to 180° round the axis. At the centre of bobbin a small magnet, carrying a mirror, is suspended by a thread. If S denote the surface of conductor, R its total resistance including that of the galvanometer, and H the horizontal component of the earth's magnetism, then q, the quantity of electricity induced, equals $\frac{2\,H\,S}{R}$. This quantity is also measured by deflection of the galvanometer needle in circuit, from which has been deduced the equation $R = 2\,G\,S\,\frac{u \tan \delta}{\tan \Delta} \cdot \frac{\pi}{\tau\,\theta}$. Knowing the length l of wire coil, and n the number of turns on the bobbin, it follows that $R = 2\,\pi\,\frac{n\,l}{\tau} \cdot \frac{u}{\tan \Delta} \cdot \frac{\tan \delta}{\theta}$. In this form, the determination of R is reduced to observing τ, the time of duration of the small oscillations of needle; the deviations θ, δ and Δ of the galvanometer needles are found by reflection on divided scales, the distances from these to the mirrors being measured.

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[1] Philosophical Magazine, May, 1881, p. 312.

The construction of the instruments is given in detail, and the various precautions discussed. Experiments were made at Versailles in the park Petit-Trianon at a great distance from all movable iron. Variations in the earth's magnetism could not be avoided, but magnetic storms were easily recognized by the appearance of the images, for then exact measurements were impossible. To eliminate the influence of temperature on the conductors, these were compared with standard resistance coils both before and after making the observations. The mercury was contained in spiral tubes placed in a bath maintained at constant temperature. The ratio $\frac{R}{R_t} = \beta$ is determined, where R is resistance of conductor in the conditions of the experiment, and R_t resistance of the standards at a known temperature t. The value of R_t being known in terms of the B. A. unit, gives $R = \beta\, R_t$ as function of the same unit. One experiment gives $\frac{u \tan \delta}{\tan \Delta}$, a correction being made in $\tan \Delta$ for the torsion of the thread and other precautions taken to get mean τ from observations. The above equation may be put in the form—

$$\frac{1}{2\,\pi\,G\,S} = \frac{1}{R}\,\frac{u \tan \delta}{\tan \Delta} \cdot \frac{1}{\tau\,\theta} = k \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Another value of $\frac{1}{2\,\pi\,G\,S} = k'$ is calculated from the dimensions of the bobbin. If the unit (B.A.U.) of the British Association used in these experiments were equal to the theoretic value of the ohm, 10^9 C.G.S., the numerical value of k, deduced from experiment, would be equal to k' calculated from measurements. This is not the case, and the quotient $\frac{k'}{k}$ gives the ratio of the theoretic ohm to the B.A. unit.

2nd. *Kirchoff's method* only differs from the preceding in that the induction $2\,H\,S$ of the earth's field is replaced by the induction of a neighbouring current which is reversed. A large bobbin and a little one were used; the former was placed horizontally on a table, and the other parallel and concentric. The large one usually served for the induced current, and the little one communicated with the galvanometer which gave the measure of the induced discharge. Special conditions were chosen to calculate M, the coefficient of mutual induction of the two circuits, from which k' was found. By electrical and magnetic measurements k is determined from the equation—

$$\frac{1}{2\,\pi\,M} = \frac{1}{R} \cdot \frac{u \tan \delta}{\tan \delta'} \cdot \frac{\tan \alpha}{\tau\,\theta} = k \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Hence M can be deduced from experiment if resistance R is known in absolute units.

After all corrections were made the mean value of the ratio $\frac{k'}{k}$ was 1·01415, and as the inverse of this represents the value of the B. A. unit, the Authors conclude

$$1 \text{ B. A. unit} = 0\cdot9861 \text{ ohm}.$$

Next the B. A. unit was compared with the resistance of a column of mercury at 0° Centigrade, at the ordinary temperature 15° Centigrade and at 12·75° Centigrade. The mean result

$$1 \text{ mercurial unit} = 0\cdot95374 \text{ B. A. unit}.$$

Hence the value of the ohm in terms of a mercurial column 1 square millimetre section, at 0° Centigrade 1 ohm = 106·33 centimetres. The results obtained by Kirchoff's method agree better among themselves, and gave the number 106·30, whilst some of the most careful and reliable experiments by Weber's method gave exactly the same value. Various reasons, such as the longer time required for an experiment, the necessity of keeping the axis exactly vertical to eliminate the action of the vertical component of the earth's field, lead the Authors to think that Weber's method is not so reliable from an experimental point of view, and they consider the value

$$1 \text{ ohm} = 106\cdot30 \text{ centimetres}$$

ought to be extremely near the truth.

W. R.

Determination of Magnetic Moments by Stilling the Vibrations of Magnets. By J. B. Baille.

(Annales de Chimie et de Physique, 1885, p. 289.)

When a little magnet oscillates at the centre of a circular conductor of radius R, a current is induced in the conductor, the electromotive force E being proportional to the magnetic moment M of the bar, inversely as the distance, directly as the angular velocity and cosine of angle ω which the magnetic axis of suspended bar makes with plane of conductor. On the other hand if a current i is passed through the conductor it acts on the central magnet according to Ampère's laws of electro-magnets. If in the relations thus deduced, absolute values of the quantities are given in C. G. S. system, equating the angular velocity in the two equations gives —

$$\frac{4\,\pi^2}{R^2} M^2 \cos^2 \omega = n \frac{E}{i} = n\,W,$$

where W is the resistance of the circuit found by applying Ohm's law, and n is the resisting-moment of the magnet for unit velocity. Placing the axis of the magnet and plane of conductor in the magnetic meridian, the amplitude of the oscillations being very small, ω is nearly zero and $\cos^2 \omega = 1$, sensibly. This only holds for a single wire. For greater action a bobbin is used, whose coefficient of sensibility $= \frac{2\pi}{R} = \frac{1}{G}$, say. To render unnecessary any correction for self-induction of bobbin, this is made with a small number of coils of wire, and the period of vibration of the magnet is long. The formula which gives the magnetic moment of the oscillating bar is then—

$$M^2 = G^2 n W.$$

The resistance W is measured by Wheatstone's bridge and standard coils, corrected for temperature which is kept as nearly uniform as possible during each observation. The quantity G, measured in centimetres, represents the mean radius of a circular conductor, which would produce exactly the same effect as the real bobbin on the magnet suspended at its centre. To find G, the bobbin is compared with an absolute galvanometer, special precautions being taken to measure the small angles by goniometers and reflecting mirrors fixed on the magnet, for vibrations of small amplitude produced by the current from one or two Daniell's cells. The coefficient n depends on the stilling of the motion, and is obtained by observing the oscillations of magnet.

The Author suspended short magnets, up to 10 centimetres in length, at the centre of a rectangular bobbin, formed by a copper wire 2 millimetres diameter wound on wooden frame and making 486 turns. The bobbin was so constructed that its self-induction was negligible, and its resistance 2·1681 ohms at 0° Centigrade. The coefficient $\frac{1}{G} = 313 \cdot 6$, hence the constant for the bobbin employed, with short magnets, was $W_0 G^2 = 10^4 \times 4 \cdot 7520$.

Corrections were made for the torsional rigidity of the suspension thread which was 0·60 metre long. When the reflected regular motion of the magnet extended over 0·50 or 0·60 metre of the scale, six successive oscillations were observed, so as to have the ratio of the amplitudes, first with open and then with closed circuit. Afterwards, when the amplitudes were smaller, another series of readings was taken, and this showed whether the stilling was regular, or if there were perturbations.

The horizontal component H of the earth's magnetism can be determined at the same time as the magnetic moment M of the oscillating magnet, knowing the moment of inertia, period of vibration, as well as the stilling of the motion of the magnet, with open and closed circuit, at a given temperature. The couple producing motion is $H M + C$, where C is the torsional couple of the thread which can be determined separately by the oscillation of a copper bar. The couple H M is known, and consequently H, since M has been given by the stilling of vibrations. By Gauss's method of finding H, it is necessary to employ two magnets and to make two different observations, separated by a considerable interval of time during which the terrestrial magnetic intensity, as well as the temperature, may have varied; whereas by the above method only one magnet and one observation is required.

In the Author's experiments the magnets employed were rectangular, so that their moments of inertia were easily calculated. The mean results for the magnetic moments M of several magnets, and the mean of the values of H, measured by the above method, are given and tabulated, showing a very close agreement with the values given by Gauss's method. The value of M for any magnet is found to decrease when the temperature rises, as one would expect. The variation of H indicates that the earth's magnetic intensity increases slightly from mid-day till six o'clock in the evening, at least for the days of observations.

W. R.

On a New Apparatus for Measuring Electric Currents.

By F. DE LALANDE.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, pp. 742–4.)

This apparatus is simply formed of a bundle of soft-iron wires, placed in the interior of a metallic areometer (hydrometer), which is immersed in a test-tube filled with water, the tube being surrounded with a solenoid traversed by the current to be measured. The tube or bar of the areometer is guided through a metal eye, to prevent friction against the sides of the tube. As constructed by Mr. Carpentier, these instruments, with a displacement of 0·1 metre, show currents of 10 to 25 amperes, or a difference of potential of 100 volts. The ammeters have a resistance of 0·01 to 0·02 ohm; the voltmeters about 1,700 ohms.

P. H.

On a Standard Volt. By A. GAIFFE.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, pp. 431–432.)

The Author has previously remarked that the density of the solution of chloride of zinc used in exciting a chloride of silver battery, influences the electromotive force, the most concentrated liquid giving the feeblest couple. A liquid containing 5 per cent. of chloride of zinc is sufficiently conducting, giving E = 1·02 volt (B. A.) or 1·01 volt (C. G. S.). It was found that the impurities of the materials of the element vitiated the results; but in operating with well amalgamated zinc, fused pure silver chloride, limpid solution of pure zinc chloride as neutral as possible, the same solution always gave the same electromotive force. A liquid weighing 107 on the densimeter appears to give the legal volt. The measurements should be made on resistances of at least 5000 ohms, because of polarization, and of the heating of the plates constituting the element.

P. H.

On the Transmission of Energy. By L. Lossier.

(L'Electricien, vol. ix., 1885, p. 629.)

The experiments conducted by Marcel Deprez, whatever else they may show, have incontestably proved that, even to small distances and under most favourable circumstances, the transmission of energy by electrical means is considerably dearer than the production on the spot by usual methods. The cost of transmitting by electricity power from Bellegarde to Lyons a distance of 123 kilometres (76 miles), would under present conditions cost at least ten times that obtained by burning coal on the spot, and it is not likely that any improvement in the future can reduce this cost 90 per cent., in order to compete with the latter. The Author proceeds to discuss the transmission of power by transportable apparatus, *e.g.*, accumulators. If it were possible to separate the peroxide of lead from the two electrodes, and transport it in a charged state, the question of transmission would be solved, because the materials would always possess the same intrinsic value, and the cost of accumulated energy would alone be required; but unfortunately this is impossible; and the extra dead weight is so large that the cost of its transport becomes excessive. Sodium, however, might be used as a magazine for transportable energy, as it is universally abundant, and as a source of electricity could be applied with the highest efficiency for the production of mechanical power. At the present moment there are two objections to its employment, the one, the high price of sodium, the other, the non-existence of any practical sodium battery. The latter objection is not discussed, because the question is now under treatment by several eminent electricians, and though no complete solution has yet been attained, the time is probably not far distant when it will be.

As regards the former, the Author first establishes by calculations detailed in the Paper that in order to compete with coal in the production of motive power, sodium must not cost more than 10 to 11 centimes per kilogram (say $\frac{1}{2}d.$ per lb.), and then proceeds to discuss the cost of manufacturing sodium by the electrolysis of common salt.

Preliminary experiments, not very complete, however, and on a small scale, in which the electrolysis of salt maintained in igneous fusion gave off at the cathode sodium in its vaporous state, which was subsequently reduced by cooling to its solid form, furnish data from which it is calculated, that, if large quantities are manufactured, using water-power as the prime motor, and an electrical current is used of such intensity as to maintain the salt in fusion without the application of heat from other sources, the price of sodium could be reduced to 24·5 centimes per kilogram (say 1·1*d.* per lb.). It must not be forthwith assumed that sodium can be manufactured at this price, but this figure is what the Author obtains from his own experiments, which can readily be repeated by any one interested in the subject.

F. J.

Experiments on the Electric Transmission of Energy between Paris and Creil. By Marcel Deprez.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, pp. 791–4.)

The Author announces the result of the experiments as very satisfactory. The length of the telegraphic line connecting the two stations is 56 kilometres; but as the current is not returned by earth it has to traverse nearly 112 metres of copper cable equivalent, as to section, to a single conductor of 5 millimetres diameter. The total resistance of this cable at 15° Centigrade is 100 ohms. The generating machine is situated at Creil. It has two rings turning in two distinct magnetic fields, each constituted by eight electro-magnets. Each ring has a resistance of 16·5 ohms and a diameter (external) of 0·78 metre. The current generated by this machine will be utilized at Chapelle by two receiving machines situated some hundreds of metres from each other. Only one of these machines is actually completed. It has, like the generator, two rings; these are of 0·58 millimetre external diameter, and have each a resistance of 18 ohms. But the experiments have been made with the receiving and generating machines side by side; the objections raised against this method have been declared by Prof. Tresca without foundation, under the condition that the actual resistance of the line is measured at each experiment. The results of the trials are the following:—

	First Trial.		Second Trial.	
	Generator.	Receiver.	Generator.	Receiver.
Speed in revolutions. . per minute	190	248	170	277
Electromotive force (direct or inverse), volts	5,469	4,242	5,717	4,441
Current . . . . amperes	7·21	7·21	7·20	7·20
Work in magnetic field . . . HP.	9·20	3·75	10·30	3·80
Electrical work in armature . HP.	53·59	41·44	55·90	43·40
Mechanical work measured on transmission dynamometer and at Prony brake . . . . HP.	62·10	35·80	61·00	40·00

Efficiency.

	First Trial.	Second Trial.
	Per cent.	Per cent.
Electric	77·0	78·0
Mechanical (commercial)	47·7	53·4

P. H.

Theoretical and Experimental Researches on Gaulard and Gibbs' Secondary Generator. By Prof. G. Ferraris.

(Memorie della Reale Accademia della Scienze di Torino, series ii., vol. xxxvii., pp. 2–73.)

These researches were undertaken to determine the very contradictory statements as to the coefficient of efficiency of the Gaulard and Gibbs' induction apparatus as employed practically in electric lighting. The result of the Author's labours were: (1) that all the experiments made to determine the coefficient of efficiency, even when these were suitable for the determination, were badly interpreted and erroneously calculated; (2) that not only from the Author's calorimetric experiments, but also from those already effected with the electrometer and electrodynamometer, rightly interpreted, the coefficient may be obtained, but that this coefficient has a different value from that hitherto accepted, and follows a totally different law to that which led to the false interpretation of the experiments; (3) finally, that from these experiments we may deduce not only the coefficient of efficiency, but also all other data useful for a complete numerical study of the phenomena that occur in the secondary generator. The first part of the treatise deals with the theoretical considerations necessary to the interpretation of the experiments; the second comprehends the actual experiments. The construction of the apparatus is described, but is well known. As a first point is studied the relation of the current-quantity to the magnetism of the core; and having found the expressions giving the values of the quantities of the primary and secondary currents, the Author determines the periodic functions of the times giving the differences of potentials between the terminals of the two coils. When the resistance of the secondary circuit is nil, the fall of potential due to the presence of the secondary generator is simply that due to the resistance of the primary coil. When this resistance of the secondary circuit differs from zero, the mean of the differences of potentials between the terminals of the primary coil is greater than that simply due to its resistance. Finally, when the secondary coil resistance is infinite, the fall of potential in the primary circuit is greatest. The difference of phases between the primary current, and the difference of potential at the two terminals of the primary coil is equal to a quarter of the total duration of this period. This deduction, which would be absolutely exact if it were possible to have a primary helix of no resistance, is verified approximately in the practical case of the secondary generators in which this resistance is always very small. The Author deems this observation very important, as useful when coming to consider the work absorbed by the secondary generator. In several experimental methods that have been followed to determine the practical efficiency of the secondary generators, the determination is made of the resistance of a conductor without self-induction substituted for the secondary generator in the primary circuit, which produces between its extremities a mean fall of potential equal to that occurring between the terminals of the primary coil for the same mean quantity of current. Now, as these experimental determinations are made either with electrometers employed as in Joubert's method, or with electrodynamometers or calorimeters, it follows that the mean currents and the mean differences of potentials are the square roots of the means of the squares. For the purpose in hand it is very important to determine the resistance of the wire, or of what these experimentalists improperly term the equivalent resistance of the secondary generator, and this determination may be very easily made by admitting that the electromotive force of dynamo-electric machine may be expressed by a single term of the trigonometrical series laid down by the Author. As to the secondary coil, if the internal resistances are negligible, the difference of potentials between the terminals of the secondary coil will be equal in dimension and phase to that existing between the terminals of the primary coil.

The Author proceeds to determine the energy absorbed and restituted by the secondary generator both as heat and electrically; also the energy absorbed by a conductor without self-induction, which, when substituted for the secondary generator, produces the same difference of mean potentials. These considerations are for secondary coils connected in simple circuit, and are subsequently corrected for the case in which the coils are connected in multiple arc.

The second part of this treatise is devoted to the discussion of experimental results and the methods already attempted for the determination of the coefficient of restitution. In his measurements the Author employs a calorimeter, and the calibration of this is described. The apparatus tested contained in the primary and secondary helices each 455 disks, the external diameter of which was 114 millimetres, the central bore being 54 millimetres. The thickness of each copper disk was 0·25 millimetre, and the height of the column 610 millimetres. The weight of the copper disks was 18,280 grams, and the total weight of the apparatus 20 kilograms. As resistances the helices had 0·276 ohm and 0·285 ohm respectively for the primary and secondary, the latter being then connected in series. The internal primary current was 12 amperes. Under these circumstances, but with the secondary circuit separated into four equal and parallel divisions, the ratio between the coefficients of practical effective restitution (or efficiency) and the theoretical was 0·99. In practice when the secondary bobbins are connected in quantity the coefficiency is slightly lower.

Several trials were made as to the influence of the structure of the core upon the power of the apparatus, and it was found that with a wooden core substituted for iron there was a deterioration of 15 per cent.

P. H.

Results of some Experiments on the Zipernowski Transformer.
By Prof. G. Ferraris.

(La Lumière Electrique, vol. xvii., 1885, pp. 145–148.)

This is a summary of some results obtained in experiments with the "secondary generator," as constructed by Zipernowski, in comparison partly with the results obtained with that constructed by Gaulard and Gibbs. The transformer was constructed to give 3000 watts. It is of annular form. The primary and secondary coils are in the interior, and constitute the core of the ring; the iron, on the contrary, is at the exterior; it is of fine iron wire, varnished, and wound on the coils of copper wire, as the Gramme coils are wound on their iron core. The primary coil, formed by a copper wire of 2·5 millimetres diameter, has two hundred and sixteen turns. The secondary coils are two in number, each composed of fifty-four turns of a copper wire of 3·5 millimetres diameter. They may be grouped at will, in tension or in quantity. The total weight of copper is 14 kilograms; that of the entire apparatus 34 kilograms. The annular "transformer" of Zipernowski has internal resistances nearly equal to that of the system of columns by Gaulard and Gibbs, but at the same time contains only two-thirds of the weight of copper, whilst it presents induction-coefficients 3·6 times greater. The Author concludes that (1) to have as great as possible surface convolutions with the smallest possible mass of metal, the metal of the centre of the turns should be spread out, and consequently wire is to be preferred to disks. (2) To obtain the greatest possible total surface of turns with the smallest length of wire, it is necessary to make a small number of turns of large diameter. (3) To give maximum efficacy to the iron core, it is better to give it a closed form. (4) To avoid Foucault currents it is necessary to interrupt the continuity of the iron perpendicularly to the currents.

P. H.

On the best Winding of Dynamo-Machines. By Dr. O. Frölich.

(Electrotechnische Zeitschrift, 1885, p. 371.)

This subject is naturally separated under two heads, viz., the winding of the armature, and the magnets respectively. The former depends on the electrical work required therefrom, and on the action of the commutator, which latter limits the allowable wire-space, and must be determined experimentally for each type of machine. Given the wire-space, the type of machine, and the velocity of rotation, the final results deduced from the fundamental equations, established by the Author in a previous Paper, show that the electrical work is independent of the winding of the armature, and is a function only of the total weight of the copper wire, and of the current flowing per unit of its sectional surface; the latter is determined by the type of machine, and must be chosen so that the heating of the armature does not become excessive.

The dimensions of the wire are determined by the special object of the installation, which arbitrarily fixes the numerical value of the electromotive force or intensity of the current, while their product, or the total electrical work, is determined by the wire-space; from these numerical values are obtained the section and number of convolutions of the wire.

As regards the magnets, the best winding is that which produces sufficient magnetism with the least expenditure of electrical work; the intensity of the current is determined by the construction of the armature, and if there is no limit to the weight of the wire, then its section should be chosen as large as possible, but in practice the diameter of the wire will be obtained by adopting a value for the current-strength allowable per unit of its section.

For "series coils" the results show that the winding is independent of the armature and its rate of rotation, but whether the resistance should be greater or less than that of the armature depends on the construction of the iron cores; it should be equal to the geometric mean of the armature resistance and that of the magnet coils when the magnetism equals half its saturation value.

For "shunt coils" the winding is, however, dependent on the armature's velocity of rotation.

In considering the merits of the two windings in their best form, the equations show that in both cases the magnetism is equal, as well as all other quantities including the weight of copper; the only difference is in the electromotive force at the terminals of the machine, and the external resistance.

For "compound coils" the requirement of a given constant difference of potential at the terminals, settles the data for the shunt winding; the dimensions for the shunt and series coils must further satisfy the conditions that the heating is equal to the minimum for either simple series or shunt coils; and also that it is divided between the windings in the same ratio as the space occupied by them.

The lost electrical work, or what is expended in the machine itself, varies directly as the resistance of the armatures and that of one convolution of a magnet coil of unit section, and as the current through the latter; and inversely as the armature function, the magnetizing function and the velocity of rotation.

When this loss is a minimum the Author considers the machine as to be in what he terms its "ideal state"; that for the best winding being termed the "normal state."

The actual solution of the values of the different electrical quantities fulfilling the conditions of the "ideal state" are worked out; but as the intensity of current in the armature may not be equal to what has been determined by the data adopted for the "normal state," the results are of no practical use.

The total lost work includes further, the mechanical effect of friction and air resistance, as well as the currents induced in the iron framing of the machine; the consideration of these leads in general to results of little practical value, as in machines of good construction these quantities are almost negligible.

The Paper closes with a comparison of the tests of two shunt-wound machines of Messrs. Siemens and Halske's construction, with what would be obtained from them if wound according to the conditions of the "normal state"; from which it appears that the efficiency in the normal as compared with the actual winding is greater, while the total work developed is less.

F. J.

On Improvements in the Efficiency of Glow-Lamps.

By WILHELM SIEMENS.

(Elektrotechnische Zeitschrift, 1885, p. 433.)

In a Paper published two years ago,[1] attention was drawn by the Author to the direction which improvements should take, in order to increase the efficiency of glow-lamps; and he now gives the results of an extended comparative test of lamps manufactured by Messrs. Siemens and Halske, with those of Edison's make. The electrical qualities of the different types of lamp, as adapted for special installations are given in tabular form, and the improved methods of manufacture are referred to, the principal being in the treatment of the filament in carburetted hydrogen, and the connection between the carbon and platinum conductor, which is effected by twisting the flattened end of the latter spirally round the former, and securing good contact by an electro-deposit of copper or nickel over the junction; which form of connection facilitates the exhaustion of the last particles of occluded gas, the presence of which causes the blackening of the interior of the bulbs and partially obscures the light. In order to eliminate this defect, the first lamps manufactured by the firm had filaments of retort carbon without this treatment, but the improved methods have allowed the introduction of carbon filaments, treated as above, with the advantages accruing therefrom. The tests were made after the expiration of each 100 hours on ten lamps of each sort, which were kept burning for 800 hours, and the superiority of the filaments prepared in this way is very evident. Thus the lamps of Edison's make of 16-candle power (nominal) diminished in intensity from 19·9 to 11-candle power, or to 55·3 per cent. of their primal value, with a mean of 13·58 candles and a mean efficiency of 159·1 candles per electrical HP.; the total decrease in efficiency being 37·7 per cent., and increase in resistance 12·7 per cent.; whereas with the carbons of special treatment, the light diminished from 17·5 to 14·5 candles, or only 17·1 per cent. with a mean of 16·24 candles, and a mean efficiency of 230·1 candles per electrical HP., the total decrease in efficiency being 12·7 per cent., and increase in resistance 4·2 per cent., three of the former and none of the latter failing during the trial. Further experiments were made at higher temperatures with similar results, but here the prepared carbons showed a loss of 39 per cent. in candle-power, so that the first trial may be considered as under normal conditions; judging by the alteration in the resistance of the Edison filaments, the loss of light in the first experiment may be ascribed principally to the deposit on the glass. The unprepared carbon filaments were subjected to still higher temperatures but were soon burnt through, so that 170 candles per electrical HP. is their highest economical efficiency.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxiii., p. 472.

These results are limited of course to lamps of this particular intensity, and where the filaments are very thin, but it is probable that with thicker filaments the superiority of the prepared carbons would be still more marked.

F. J.

The Theory of the Electro-Magnetic Telephone Transmitter.

By E. Mercadier.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, pp. 744, 1001.)

This is a study of the part played by the iron or steel diaphragm of a telephone as to its elastic movements, and as to the transformation of mechanical into magnetic energy resulting from its movements. The vibrations of a diaphragm are not those due to free elastic movement. Interposition between the diaphragm and the source of vibratory movements, of a plate of any substance, alters nothing of its telephonic qualities. Destruction of its harmonic sounds, as a plate, does not interfere with its telephonic character. The conclusion appears to be that the mechanism in virtue of which telephonic diaphragms execute their movements is analogous to, if not identical with, that by which any solid bodies, as a wall, transmit vibratory movements. The Author has been able to suppress completely the rigidity and elasticity of the diaphragm, and has obtained feeble reproduction of articulate speech with iron filings thrown on to a sheet of paper placed on the end of the electro-magnet. With iron filings laid over a disk of card, and with increase of the intensity of the magnetic field, effects as intense as with an ordinary transmitter, and also reversible, have been obtained. These observations apply equally to telephonic receivers.

P. H.

A new Solenoid-Galvanometer. By R. Scharfhausen.

(Centralblatt für Electrotechnik, vol. vii., 1885, p. 498.)

This instrument, the graduation of which is empirical, and which is designed rather for practical use than for scientific work, consists of a glass U-tube with legs of different diameters partially filled with mercury; in the larger tube, the lower part of which is surrounded by a solenoidal coil, floats an iron core. The displacement of the mercury by the latter will be increased and vary according to the intensity of the current traversing the coil, and thus alter the level of the mercury in the other tube. The advantages claimed are that the readings are perfectly "dead-beat," and that the motion of the core can be multiplied to any extent by merely increasing the ratio between the diameters of the two tubes.[1]

F. J.

The Electrolysis of Salts. By A. Renard.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, pp. 747–9.)

From his experiments, the Author arrives at the following conclusions: (1) In sufficiently dilute solutions, the quantity of metal precipitated is proportional to the concentration of the liquid; (2) the proportions of the metal deposited are in the same ratio as their equivalent atomic weights; (3) according to Faraday's law, the quantity of metal precipitated being proportional to the quantity of current, the conductivity of solutions containing equivalent atomic weights of metal is the same, as demonstrated by Mr. Bouty[2] by a direct method.

P. H.

Electromotive Force of Cells containing Saline Solutions.

By B. C. Damian.

(Annales de Chimie et de Physique, 1885, p. 289.)

The variations of the electromotive force with time, in single-liquid cells, were observed in more than two hundred different combinations. Mascart's electrometer and the standard Latimer Clark cell were used to measure the electromotive force.

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[1] A similar instrument has been previously described by P. Meardi, in the Giorno; and with the addition of two platinum wires inserted in the smaller tube, between which the mercury makes contact, on reaching a certain level. Mr. Millar, of the Gérard Company in Paris, claims the invention of this instrument as well.—Ed. Centralblatt für Electrotechnik.

[2] Annalen de Chimie et de Physique, vol. iii., p. 433.

It is found in general the electromotive force diminishes for sulphates, nitrates, and carbonates. The variations are very feeble for the cell made up of zinc, copper and sulphate of magnesia. During ten months the electromotive force varied between 1·06 and 1·04 volt. The solution remains perfectly limpid, and after nine months, the surface of the metals is as clean as on the first day. In this cell the concentration of the solution has no appreciable influence on the electromotive force, which is also sensibly independent of the temperature from 6° to 35° Centigrade. In a cell set up six months the numbers obtained varied between 1·052 and 1·068 volt for this range of temperature.

Even in a circuit, closed during three days, with large external resistance of 15,000 ohms, the diminution was only 0·04 volt. The current was very feeble, about 0·00007 ampere. Introducing an external resistance, 20,000 ohms, the electromotive force of this cell, at first 1·454 volt, fell in forty-eight hours to 1·423 volt. Again the experiments show that for elements made of zinc, copper, and a saline solution, the electromotive force is nearly the same when the liquids belong to the same class of salts; this is not true of cells formed with lead and platinum.

In the cell of amalgamated zinc, platinum, and dilute sulphuric, the maximum of electromotive force, 1·345 volt, is given at 15° Centigrade, when the solution contains 30 to 100 of acid, and has density 1·223. In fact, it appears that in general with cells having zinc plates immersed in acidulated water, the maximum electromotive force is obtained by using sulphuric acid of this strength.

W. R.

On the Behaviour of Liquid Atmospheric Air.

By Sigmund von Wroblewsky.

(Wiedemann's Annalen der Physik und Chemie, 1885, p. 134.)

Very often atmospheric air appears as a simple gas. If it is changed into a liquid it also seems, on merely superficial observation, to behave as a simple gas, but many complicated phenomena occur, leading to the inference that the behaviour of air is due to its constitution as a mixture of two gases.

The Author has succeeded in obtaining from atmospheric air the same phenomena that appeared by compressing a mixture of five parts by volume of carbonic acid, and one part by volume of air. In experimenting upon atmospheric air under compression, the Author obtained two distinct liquids, appearing in the vessel to be clearly separated by a meniscus plane, and each of these liquids was separately drawn off and analyzed.

If atmospheric air is exposed to a pressure of about 40 atmospheres, the meniscus of the liquid air generally commences to form when the pressure has sunk to about 37·8 atmospheres, and when the galvanometer registers a temperature of about $-142°$ to $-143°$ Centigrade, but cases have been known where the formation of the meniscus commences at a higher temperature. If the pressure in the apparatus be much diminished, the result may be described by a tension-curve, some idea of the form of which may be obtained by the figures in the annexed Table :—

θ.	d.	θ.	d.	θ.	d.	θ.	d.
−144·5	31·42	−147·25	27·39	−150·1	23·67	−152·2	20·99
145·0	30·61	148·1	26·37	150·3	23·51	152·25	20·46
146·2	28·67	148·3	26·02	150·45	23·36	153·0	19·36
146·5	28·24	148·6	25·78	151·25	22·2	153·35	18·61
147·0	27·8	149·4	24·67	152·1	21·0	154·0	17·55

θ signifies the temperature, d the pressure in atmospheres.

These figures are, however, subject to great variations.

The rapid changes in the behaviour of liquid air appear more distinctly when it is subjected to boiling under the pressure of 1 atmosphere. The boiling-temperature then rises constantly, and the variation of the boiling-point proves that the liquid is every moment deprived of more and more of its nitrogen. The following Tables, which prove the alterations in the boiling-temperature, may be of interest :—

FIRST EXPERIMENT.

−191·4	190·4	190·3	189·6	189·4	188·9
188·7	188·7	188·6	188·4	188·2	188·0
187·45	187·1	..	..	..	..

SECOND EXPERIMENT.

−190·8	189·7	189·2	188·9	188·5	188·15
188·15	188·15	188·15	188·05	..	..

Both experiments show quick rising in the boiling-temperatures. By the second experiment, however, the liquid commences to boil at a higher temperature than by the first, because the former is poorer in nitrogen than the latter.

The phenomena appear still more striking when the atmospheric air is evaporated in a vacuum. The following figures will describe this :—

w' the reading on the galvanometer in centimetres.
w the deflection, calculated after three readings in centimetres.
θ the corresponding temperature.
d' the tension of the vapours in centimetres, mercury pressure.

The reading first began when the tension was below 1 atmosphere.

FIRST EXPERIMENT.

w'.	w.	θ.	d'.	w'.	w.	θ.	d'.
68·90	..	..	..	12·40	28·325	−196·95	..
12·72	28·14	−195·02	..	69·18	28·428	198·0	4·4
69·10	28·25	196·2	..	12·25	28·47	198·5	..
12·48	28·335	197·1	..	69·20	28·488	198·65	4·1
69·20	28·375	197·5	..	12·20	28·505	198·8	4·0
12·42	28·385	*197·6*	..	69·22	28·515	*198·95*	..
69·78	28·385	*197·6*	..	12·22	28·483	198·6	3·8
12·40	28·383	*197·6*	..	69·15	28·445	198·2	..
69·15	28·375	197·5	16·1	12·30	28·418	197·9	3·6
12·40	28·375	197·5	..	69·12	28·41	*197·85*	..
69·15	28·365	197·38	15·2	12·30	28·41	*197·85*	3·5
12·44	28·343	197·15	..	69·12	28·43	198·05	..
69·10	28·315	196·85	14·6	12·22	28·465	198·5	3·2
12·50	28·295	196·65	..	69·18	28·505	198·75	..
69·08	28·295	196·65	12·4	12·12	28·548	199·28	..
12·48	28·295	196·65	11·0	69·25	28·575	199·55	3·0
69·06	28·285	196·55	10·0	12·08	28·598	199·8	..
12·50	28·245	196·1	..	69·30	28·62	200·0	2·8
68·92	28·235	*196·0*	..	12·04	..	..	..

SECOND EXPERIMENT.

w'.	w.	θ.	d'.	w'.	w.	θ.	d'.
12·40	..	..	..	12·02	28·348	−197·2	5·0
68·50	28·15	−195·1	..	68·78	28·42	197·75	..
12·00	28·305	196·8	..	11·90	28·47	198·5	4·0
68·72	28·385	197·6	..	68·90	28·52	199·0	..
11·90	28·418	197·9	..	11·82	28·54	199·2	3·8
68·75	28·425	*198·0*	..	68·90	28·54	199·2	3·6
11·90	28·425	*198·0*	..	11·82	28·54	199·2	..
68·75	28·42	197·95	16·2	68·90	28·54	199·2	..
11·92	28·415	197·9	15·0	11·82	28·545	*199·25*	..
68·75	28·41	197·85	14·0	68·92	28·535	199·15	3·4
11·94	28·405	197·8	..	11·88	28·503	198·75	..
68·75	28·405	197·8	13·0	68·85	28·48	*198·6*	3·2
11·94	28·413	197·9	12·0	11·90	28·488	198·7	3·0
68·78	28·405	197·8	11·0	68·90	28·52	199·0	..
12·00	28·365	197·4	10·0	11·82	28·55	199·3	..
68·68	28·32	196·9	..	68·94	28·57	199·5	2·7
12·08	28·293	*196·6*	8·0	11·78	..	..	..
68·65	28·30	196·7	..	..	..	..	..

These figures show that here the tension-curve varies much more than would be the case with a simple gas. The temperature falls first simultaneously with the diminution of pressure, viz., when the pressure has reached 16 centimetres. It then attains to the first minimum, which by first experiment amounts to −197·6°, by second to −198°. Then with further decrease of pressure the temperature commences to rise, and at a pressure of about 9 centimetres a maximum is attained, by first experiment of −196°, by second of −196·6°. By further decrease it falls again; at the pressure of 3·5 centimetres it rises a little once more, and at the pressure of about 2·5 centimetres it is only a fractional part of 1° lower than the temperature shown by pure liquid oxygen under the same pressure. Liquid atmospheric air, therefore, still only contains a very small quantity of nitrogen. These variations in the tension-curve distinctly prove that both the ingredients of the air do not evaporate in the same way, and that the temperatures which the liquid registers depends on the momentary composition.

Now as to the trials by which the Author has succeeded in separating the atmospheric air into two liquids, separated by a meniscus plane, two experiments may best illustrate them.

First experiment.—The pressure in the apparatus falls slowly, at the same time the temperature of the liquid air changes thus:—

d	33·55	33·46	33·35	33·14	32·94
θ	−143·7	143·8	143·9	144·0	144·0

The compression-apparatus is shut off. The pressure and temperature are then relatively

d	40·7	40·37	39·87	39·53	39·19	38·85
θ	−142·3	142·2	142·2	142·25	142·49	142·55
d	38·54	38·19	37·89	37·6	..	..
θ	−142·55	142·55	142·55	142·55	..	..

At this moment the meniscus appears above. The pressure and temperature are then—

d 37·34 37·11 θ − 142·56 −142·65

At this moment the former meniscus is observed. The volume of the upper liquid is apparently more expanded. It is noticeable then that d − 36·95, θ − 142·5. After some time

d 36·15 36·05 θ − 142·2 − 142·25

The separating plane at this point gets very distinct. The bubbles commence to ascend from it, and obscure the clearness of the upper liquid. The galvanometer registers

θ − 142·3 − 142·35

The separating plane now disappears, and the whole liquid looks homogeneous. Readings are now taken, d 34·93, θ − 142·35. After some time d 32·68, θ − 144·0. Now the gaseous air is admitted again, until the meniscus disappears. It appears again at a pressure of 37·4 atmospheres, and at a temperature of −142·6. The separating plane is again observed at 36·05 atmospheres, and −142·7. It gets very distinct at 35·49 atmospheres and −142·7, and disappears again at 34·05 atmospheres and −142·8, and so on.

Second experiment.—This trial is interesting, because by it the same phenomena occur at higher temperatures. In this experiment very small quantities of air were taken. After the meniscus appeared at 37·6 atmospheres and −140·8 Centigrade, the plane which separated the two liquids was observed at 36·95 atmospheres and −140·9. It became indistinct at 35·35 atmospheres and −141·5. After the meniscus had been caused to disappear, by leading the gas in, it appeared again at 37·5 atmospheres and −140·4. The separating plane was observed at 37·33 atmospheres and −140·4. It disappeared again at 36·4 atmospheres and −140·5° Centigrade. An analysis of the two liquids gave the following in parts per volume:—

OXYGEN.

	Lower Liquid.	Upper Liquid.
1st essay	21·3	18·7
2nd „	21·5	18·5
3rd „	21·28	17·3

Time did not permit the Author to ascertain if the composition of both liquids was variable or not. The excess of oxygen in the lower liquid is explained by reason of its being easier changed to a liquid than nitrogen. That the two liquids are capable of separation is accounted for by their different specific gravities, because the air richer in nitrogen is specifically lighter.

C. A.

On the Dimensions of Molecules and their Relative Distances.

By Prof. R. CLAUSIUS, Hon. M. Inst. C.E.

(La Lumière Électrique, vol. xvii., 1885, pp. 241–244.)

This is a letter in reply to an epistolary communication of certain arguments as to the dimensions and relative distances of molecules advanced by Mr. Jules Bourdin, and published in La Lumière Électrique of May 30th, p. 419. The Author refers to the laws enunciated by him in 1882—that the mean path of the molecules, multiplied by eight, is to their diameter as the total volume occupied by the gas is to the volume occupied by the molecules; and to the fact that if the gas departs from the law of Boyle[1] and Gay-Lussac, the departure is due to several causes, one of which is that the volume actually occupied by the molecules cannot be neglected as compared with the total volume of the gas. The Author concludes that in order to explain the propagation of luminous waves across space it is requisite to admit the existence of a matter susceptible of more subtle division than the ponderable gases; this is the matter to which at present the name ether has been given; the Author deems it no other than electricity.

P. H.

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[1] Mariotte in the original.

On a Recorder of the Calorific Intensity of Solar Radiation.

By A. Crova.

(Comptes rendus de l'Académie des Sciences, vol. ci., 1885, pp. 418–421.)

In the course of his researches on the diurnal and annual variations of solar radiation, the Author has remarked departures and causes of uncertainty that he has only been able to explain by continuously recording the intensity. The problem to be solved was to record the indications of an actinometer giving the calorific intensity of the solar radiation, this apparatus receiving the solar radiations directly, that is without interposition of any transparent plate, and protected from perturbing action of the wind. The recording actinometer is formed of two parallel disks, each composed of two plates of iron, constituting a thermo-electric element enclosed in a thin brass-tube; one of the joints is obscured; the other receives the solar rays normally to its blackened surface in the axis of the tube, which is furnished with five aluminium diaphragms having decreasing openings. This tube is mounted on an equatorial movement to maintain its axis in the direction of the solar rays. The disk receives the sun only on a small part of its surface, but in consequence of the Peltier phenomenon, the effect is the same as if the quantity of energy contained in the section of the solar pencil were uniformly expanded. The Author has verified this fact by concentrating with a convergent lens, a solar pencil on this actinometer; the current obtained is the same whether the pencil falls on the plate at the point of principal focus or whether the lens is approached so as to cover the whole surface.

The two extremities of the element and the points of junction of the wires are soldered galvanoplastically by a deposit of copper to a circuit connected to a galvanometer placed in a darkened room. The indications of the mirror galvanometer are recorded photographically.

P. H.

The Great Alpine Tunnels and Subterranean Temperatures.

(Gesundheits-Ingenieur, 30th Sept., 1885, p. 551.)

Among the many difficulties encountered in the execution of the works connected with the St. Gothard Tunnel, one of the greatest was the prevalent high temperature in the central section, 5 kilometres in length. The maximum temperature observed in carrying out the Mont Cenis Tunnel was 29·5° Centigrade (85° Fahrenheit), while a mean temperature of 29° (84·2° Fahrenheit) was confined to a section of 500 metres in the middle of the tunnel. During the execution of the St. Gothard Tunnel, at a distance of 4⅓ kilometres from the southern end, and at 5⅓ kilometres from the northern extremity, this temperature had been already attained, and at the time when through communication was established (February 29, 1880), the mean heat, extending over a section of the works, 5 kilometres in length, had risen to 31° (87·8° Fahrenheit), and on several occasions attained the maximum temperature of 35°. The air, which was considerably rarefied and much laden with moisture, proved very injurious to health—as many as 60 per cent. of the staff being on the sick list—while the horses died at the rate of 10 per month at each end, from a species of lung disease. As a necessary consequence the wages had to be raised 25 per cent., the working hours were reduced from seven to five per diem, and the costs were enormously increased. The limit of temperature at which underground work is still possible could not with certainty be ascertained. According to Du Bois-Reymond it may be assumed that at 50° (122° Fahrenheit), in air saturated with moisture, prolonged existence becomes impossible, though it appears very probable that even at 40° (104° Fahrenheit) this may be the case. The opinion of Dr. Giaconne that in the above works the limits of human endurance had been reached was shared by the staff.

The common law of increase of temperature in proportion to depth does not apply in tunnel construction; Dr. Stapff[1] has, from observations on the St. Gothard Tunnel, propounded the theory that the increase of temperature above the mean temperature at the earth's surface, is represented by the formula $t = 0{\cdot}02068\,h$, or $t = 0{\cdot}02159\,n$, where h is the perpendicular height of the superincumbent mountain, or n is the distance inwards of a given point, on the plane passing through the longitudinal section. Stockalper has, in a different way, calculated the temperatures likely to be encountered in carrying out the Simplon and Mont Blanc tunnels as follows, reckoning from the northern end:

SIMPLON.

(About 22 kilometres in length.)

		Metres.	Probable temperature (Centigrade). °
4·0	kilometres in and a height of	1,400	30
6·5	,, ,, ,,	1,000	33
9·0	,, ,, ,,	2,050	36
14·7	,, ,, ,,	680	31
17·0	,, ,, ,,	1,734	31
18·0	,, ,, ,,	1,100	25

MONT BLANC.

(18·5 kilometres in length from Taconnaz to Prés, St. Didier.)

		Metres.	Probable temperature. °
4·0	kilometres in and a height of	1,550	33
5·0	,, ,, ,,	2,800	50
6·0	,, ,, ,,	3,000	53·5
8·0	,, ,, ,,	2,600	46
10·5	,, ,, ,,	1,450	31
13·0	,, ,, ,,	200	15

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[1] Minutes of Proceedings Inst. C.E., vol. lxii., p. 399.

From these calculations he is persuaded that the execution of these tunnels will be impracticable on the systems hitherto adopted, and that it will be necessary to take very special precautions to guard against these excessive temperatures. Among other plans to be employed he mentions a bratticing to convey in air along the floor of the tunnel, use of compressed air for boring, introduction of ice into the workings, as also of streams of cold water; employment of quick lime to absorb the moisture, &c.; and he concludes that though it may be possible under certain conditions to execute the projected tunnel through the Simplon, the possibility of carrying out the proposed Mont Blanc Tunnel, under any circumstances, is open to the gravest doubt.

G. R. R.

Carbonic Acid in the Liquid and the Solid state.

By Dr. RAYDT, of Hanover.

(Mittheilungen der Verein zur Beförderung des Gewerbefleiszes, July 6, 1885, p. 184.)

The employment of liquefied carbonic acid gas for various manufacturing operations has resulted in the development of a new industry. For many years after Davy and Faraday had succeeded in effecting the liquefaction of this gas, and even after Natterer of Vienna had devised a special apparatus for the purpose, the process was regarded merely in the light of a lecture experiment, and was only usefully employed for the production of very low temperatures. The Author first turned his attention to the subject in connection with the plans for raising the armour-clad ship, the "Grosser Kurfürst" in 1878. His first practical experiment in this direction was carried out at Kiel in 1879, when, by means of about 40 kilograms of liquid carbonic acid enclosed in a receiver, to which was attached an empty balloon formed of sail-cloth coated with india-rubber, a block of stone weighing 316 centners (16 tons) was raised to the surface in eight minutes after the tap opening the communication from the receiver to the balloon was turned by a diver. The success of this experiment led to the proposal to found a company for the raising of sunken vessels on this system, but the scheme came to nothing, partly because of the difficulty at that time of producing the liquid in large quantities at a cheap rate.

Subsequently, Mr. F. A. Krupp, of Essen, used this fluid for the production of low temperatures for shrinking out the cores of cannons, and for the compression of molten metal in the moulds. He obtained in this way a pressure of 75 atmospheres, and was able to produce much more solid and dense castings than before. In consequence of the steady improvement in the pumping apparatus employed at Essen for the compression of the gas, and the production of the liquid in large quantities, it became possible, owing to its cheapness, to make use of it for beer-raising and in the manufacture of artificial mineral waters.

In process of time the firm of Kunheim and Company, of Berlin, took the matter in hand and turned the scientific facts to industrial account. Their business was taken over by the Berlin Company for the Carbonic-Acid manufacture, which has been most successful, and now produces daily eighty cylinders, each containing 8 kilograms, or 640 kilograms (1,411 lbs.) of liquid carbonic acid, equal to 320,000 litres of gas.

This is employed for beer-raising in the same way as compressed air was formerly made use of. The cylinder of liquid is attached to a receiver, and on turning the tap connecting the two vessels the liquid rushes in, and expands in so doing into the gaseous form. In a few seconds this gas attains a pressure in the larger vessel of 1½ atmosphere, and the tap is then closed. The pipes conducting to the beer-casks are subsequently opened, and the carbonic acid gas flows into the casks with a pressure sufficient for the drawing off of the beer. As only pure carbonic acid gas passes into the beer-casks all putrefactive processes are avoided, and the beer is drawn in a bright and sparkling condition. Eight kilograms of the compressed gas are sufficient for the delivery of 24 to 30 hectolitres of beer.

The pressure exerted by liquid carbonic acid varies in accordance with the temperature; thus:—

at − 79° Centigrade the pressure = 1·2 atmosphere.
„ − 10° „ „ = 27·5 „
„ + 5° „ „ = 40·5 „
„ + 20° „ „ = 58·8 „
„ + 30° „ „ = 73·0 „

All danger in the storage of the compressed gas is avoided by the testing of the cylinders (which are made of wrought iron) to 250 atmospheres before they are filled, and the receivers are furnished with a safety-valve, weighted to a little over 1½ atmosphere pressure, and these are all tested to 5 atmospheres. Although between six and seven thousand cylinders of the liquefied gas have been sent out, and have been used for the most part by unskilled persons, there have been no accidents of any kind. The price of a cylinder of the liquid is 16 marks (16*s*.).

By removing the pressure or allowing the liquefied gas to escape into a woollen bag, an intense cold is produced, and a sufficient amount of heat is abstracted to cause a portion of the remaining liquid to freeze into crystals, resembling snow. These frozen needles can be brought, by means of slight pressure, into solid lumps resembling chalk, which are specifically heavier than water, and which by increased pressure can be obtained of a specific gravity of 1·5. By surrounding this solidified carbonic acid with a bad conductor it may be preserved for as much as fifteen hours, during which time it is of course being gradually dispersed in the gaseous form. The Author mentions the importance of the refrigeration, obtained by the transition from the liquid to the gaseous form, to the brewer and the mineral-water maker, and the process of employing this material for the manufacture of mineral waters is explained. Various systems of using the liquid carbonic acid for fire extinction are next considered, and the Author explained some practical experiments with a small model fire-engine constructed for the purpose. The future use of the liquid as a means of saving leaky and sinking ships is dwelt upon, and the facility of its preparation from the free carbonic acid gas, issuing from the earth in certain districts, is mentioned.

The scientific aspects of the subject are treated of at some length, and the Author insists upon the importance of the fact that the boiling-point of this substance is lower than its freezing-point, as it begins to boil in an open vessel at − 78·2° Centigrade, but it freezes at −58°. It can thus pass at once from the solid to the gaseous form without becoming liquid, which is contrary to the general behaviour of other substances, which first melt and then pass over from the liquid to the gaseous condition. Some experiments by Mr. F. A. Krupp on the amount of pressure exerted by the liquefied gas are quoted in conclusion.

1. At a constant temperature of 100° Centigrade, in a vessel capable of containing 134 grams of the liquid, when the vessel was filled to the extent of 36·5 grams, the pressure was 90 atmospheres; when filled to 88·5 grams, the pressure was 247 atmospheres; and when quite full the pressure was 760 atmospheres.

2. With the receiver full and the temperature gradually raised, at 105° Centigrade the pressure was 775 atmospheres; at 143·5° Centigrade the pressure was 988 atmospheres, and 161° it was no less than 1,100 atmospheres.

G. R. R.

On the various methods of Estimating the volume of Carbonic Acid Gas in the Atmosphere.

By Prof. Dr. Wolpert, of Nuremberg.

(Gesundheits-Ingenieur, 15th Oct., 1885, p. 569.)

The opinion formerly prevalent that the use of air for breathing purposes (which gradually abstracts from it the oxygen, and adds to the amount of carbonic acid gas) led to its rapid deterioration is scarcely correct, as it has been demonstrated that the oxygen may be reduced to two-thirds of its normal quantity, and the amount of carbonic acid may be increased to ten volumes per mille, without causing any discomfort, or being productive of any injury to health. Long before this could happen, however, the air in question would have become highly impure, owing to its being impregnated with foul smelling organic exhalations, and the various excretions from the skin and lungs. As it is difficult accurately to estimate the amount of these impurities, the increase in volume of carbonic acid, which appears to be fairly proportionate to the degree of pollution, has been proposed by Pettenkofer as the standard for the determination of the deterioration of the atmosphere, due to respiration and perspiration. Estimations of the contents in carbonic acid gas in the atmosphere have therefore to be undertaken very frequently, and it is important that rapid and easy methods of arriving at fairly reliable results should be studied. Very great accuracy may be obtained by the method proposed by Pettenkofer, which depends on the absorption of the gas by means of baryta-water, titration with oxalic acid, and the employment of rosolic acid to indicate neutralization. In carrying out this process it is usual to employ glass bottles, holding from three to nine litres. The air to be tested, having been properly collected, 100 cubic centimetres of baryta-water are introduced into the vessel and, after the absorption of the carbonic acid, twenty centimetres are taken out by means of a pipette and titrated with standard oxalic acid, coloured with a few drops of rosolic acid. The standard acid is of such strength that each cubic centimetre represents one milligram of carbonic acid. The quantity of acid used for titration is thus a direct measure of the volume of carbonic acid already absorbed by the baryta-water and the calculation of the proportionate volume in the atmosphere is readily accomplished. The same principle has been employed for the process of Dr. Hesse, but the size of the apparatus has been so much reduced that it can all be packed in a very small compass. A small and simple apparatus, the action of which is described, and for which baryta-water is also used, has been devised by Lunge. In Rüdorff's process the carbonic acid is absorbed by standard caustic potash, with arrangements for measuring the volume of the gas. The apparatus consists of a three-necked bottle, with filling tube and pipette, which is sunk in water to avoid, as far as possible, the influence of changes of temperature. The plan recently proposed by Dr. Blochmann[1] depends upon repeated fillings of a bottle with the air to be tested and the absorption of the gas by lime water. Both with this apparatus, and also with that of Lunge the repeated fillings and shakings are slow and tedious. The Author has introduced a pocket apparatus invented by him in 1882, consisting of a glass cylinder, having at the bottom a number to serve as an indicator. An india-rubber ball furnished with a glass tube is filled with air, and after charging the cylinder with saturated lime-water, the air is slowly pressed out of the ball, and allowed to bubble through the lime-water in the cylinder. This operation is repeated until the liquid becomes so milky that the number at the bottom can no longer be read. The number of fillings of the ball are reckoned, and the corresponding volume of carbonic acid gas is read off from a table. This plan is quite accurate enough for ventilation purposes, but the Author intends to produce a better apparatus, in which the portions in glass and india-rubber will be replaced by brass, and he hopes that still more reliable results will be obtained.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxxi., p. 384.

G. R. R.

The Resistance of Built-up Guns, and the Shrinkages adapted to their Construction. By Lieut. ROGERS BIRNIE, Jun., U.S. Army.

(Notes on the Construction of Ordnance, Washington, Sept. 14, 1885.)

In introducing the subject of this Paper the Author lays down the general principle that, "In whatever state the system of a built-up gun may be considered, none of the fibres of any cylinder in the structure should be elongated or contracted beyond the elastic limits of the material." And considering the state of that, he says that the shrinkages of the outer cylinders must be limited to retain uninjured the elastic properties of the metal at the surface of the bore of the tube.

In a foot-note, however, he admits that it is an open question whether the compressions of the bore may not with advantage be carried beyond their elastic limit.

In Parts I. and II., following the methods of Virgile and Clavarino, but assuming that there is no longitudinal traction, he deduces formulas for the equilibrium of a cylinder of a constant elasticity, and forming part of a built-up gun. He states that owing to the variableness of the longitudinal traction and its unequal distribution, it would be extremely difficult to find equations expressing the equilibrium of the systems in action; but he maintains, that no great error will be committed in adopting the values of the allowable interior tensions from the different equations which omit the longitudinal tractions altogether.

In treating in Part II. of the practical limit for the compression of the bore, he says that, "if it can be shown by practical methods that this can be carried with safety beyond the elastic limit of the material the value of the interior pressure may be increased; but in estimating the amount of this increase he assumes that the modulus of elasticity remains unchanged after the elastic limit is exceeded. This assumption is, however, at variance with fact.

He then deals with the calculation of the shrinkages, but always on the assumption of the longitudinal tractions being neglected, and proceeds to determine the varieties of pressures and changes of dimensions incident to the several stages of construction.

His next step is to determine the resistance of a gun constructed with shrinkages, varying from the theoretical.

In concluding this part of his subject the Author remarks that the fundamental principles of his equations are: 1st, that the system is not acted upon by any longitudinal tension; 2nd, that the elastic displacements of the metal are directly proportional to the stresses;

3rd, that the value of the coefficient of construction is $m = \frac{1}{3}$ as deduced by Wertheim.

The first of these assumptions is practically untrue, and the second only holds good up to the elastic limits, consequently the formula can only be taken as approximately true.

Part III. contains a summary of the preceding formulas as applied to a gun structure composed of from three or two concentric cylinders; and Part IV. a numerical example of the same as applied to an 8-inch gun composed of four concentric cylinders, the breech-block being carried in the second cylinder.

In an appendix the Author gives an investigation by Lieutenant William Crozin, U.S. Army, of the stresses in a hollow-tube subjected to any internal and external pressures, and a diagram of the equations arising therefrom.

J. A. L.

[In the brief summary above given it will be seen that the Author follows very closely the methods of Virgile and Clavarino, which again are fundamentally those of Dr. Hunt and Messrs. Longridge and Brooks, published many years ago.

This is not the place to enter into any criticism of those methods, but it may be remarked that all that has been written on the subject goes to prove the extreme difficulty of constructing built-up guns, arising chiefly from three causes; the first is the complication resulting from the mixture of the longitudinal strains and the tangential strains in the same material; the second is from the uncertainty of the distribution of the longitudinal strains, which is certainly not uniform, over the sectional area of the tubes or jacket. These two difficulties are entirely avoided in the construction advocated by Mr. Longridge, whereby the two strains are separated, and each borne independently by its own material.

The third difficulty arises from the uncertainty which exists about the behaviour of a metal cylinder when its inner surface is subjected to a compression exceeding its elastic limit. This difficulty can only be satisfactorily met by a series of experiments which, though easy and simple, have not as yet been made.—J. A. L.]

On the Internal Strains set up by Tempering Tubes and Hoops for Ordnance.

By — HARMET, Chief Engineer to the St. Etienne Iron and Steel Works.

(Bulletin de la Société de l'Industrie Minérale, vol. xiv., 1885, p. 337.)

The enormous internal strains set up in tempering steel tubes and hoops may, according to their direction, either increase or decrease the normal powers of the steel to resist the force of explosion. In the method ordinarily employed, the simultaneous quenching of the interior and exterior surfaces sets up two strains opposite in direction, and consequently ill adapted to resist the central and unique strain of the charge.

To obtain the best result from a given weight of material, each portion of the tube should be so disposed as to offer an equal amount of resistance to the central expansive force of the powder. In the case of an untempered tube, free from initial strains, which may be considered as composed of a number of concentric zones of homogeneous material with diameters $d_1, d_2 \ldots . d_n$ (numbered from the innermost zone), if a represents the increase of the interior diameter caused by the explosion of a given charge, then the strain borne by zone 1 will be—

$$F_1 = \frac{\pi (d_1 + a) - \pi d_1}{\pi d_1} = \frac{a}{d_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the strain borne by zone n will be—

$$F_n = \frac{a}{d_n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and consequently less, since d_n is greater than d_1; and generally the strain

$$F = \frac{a}{d} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

borne by any zone will be inversely proportional to its diameter. It follows that *the resistance of a tube free from initial strains is* not *proportional to its thickness.*

From formulas (1) and (2)

$$F_1 - F_n = a \left(\frac{1}{d_1} - \frac{1}{d_n} \right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, in general terms,

$$F_1 - F = a \left(\frac{1}{d_1} - \frac{1}{d} \right) = A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

showing that, for any two zones, the difference of strain increases with a; that is to say, *the heavier the charge, the more unequal are the strains borne by the interior and exterior zones;* also, that in a tube of unlimited thickness, the exterior zone would do no work at all.

In the above formulas it is supposed that the increase of interior diameter of the innermost zone at the moment of explosion is communicated exactly to all the rest; but in fact, owing to the compression of the metal, and the heat developed at the moment of firing, the differences of strain are greater than those indicated by the formulas.

As this defect cannot be met by any alteration in the action of the powder, or in the circular form of the tube, it is desirable to set up definite initial elastic strains in the body of the metal, to compensate for the injurious effects of the unequal diameters of the zones, and to relieve the inner ones by throwing more work on the outer ones, in proportion to their distance from the centre.

Formula (5) gives the difference between the maximum strain F_1 and the strain F, for any particular zone. This difference A exactly represents the permanent initial strain that must be imposed on that zone, so that the total strain upon it at the moment of firing may be equal to that on zone 1.

From formula (5) may be deduced—

$$A_n = F_1 - F_n = a\left(\frac{1}{d_1} - \frac{1}{d_n}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

showing that the desired initial tensions at each point are complementary to the strains produced by the powder alone, and also that their values are always *positive,* as compared with the elastic state of zone 1; so that although the *absolute* elastic state of any zone may be a compression if zone 1 is itself in compression, yet *relatively* to zone 1 it must be in the state of tension indicated by formula (6).

In order to investigate the possibility of producing the required initial strains, these imaginary zones must be supposed to be actually separated, and shrunk on to each other with an elastic grip increasing successively from 1 to n. Thus all the zones will be in *relative and positive tension* on zone 1, as above required. But taking into consideration the *absolute values* of these strains, each action setting up a reaction, one-half are found to be positive, the other negative; that is to say, the zones from 1 to $\frac{n}{2}$ are in absolute compression, or negative tension, and left to themselves would have a tendency to expand, while the rest are in absolute positive tension, with a natural tendency to contract.

This state of absolute compression of the zones from 1 to $\frac{n}{2}$, permitting them to oscillate about their point of equilibrium while doing their work, is an advantageous one; and if it can be arranged that the diminution of diameter under this compression shall be $\frac{a}{2}$, the movement during explosion will be limited by two circles with diameters $d_1 + a$; while the intermediate circle $d_1 + \frac{a}{2}$ would represent the zone in equilibrium.

Out of numerous experiments on tubes and hoops, the following may be selected for examination, one tube having been completely quenched in cold water, the other quenched on the interior surface only. A length of about 30 millimetres ($1\frac{3}{16}$ inch) was cut off each tube, the interior diameter being about 83 millimetres ($3\frac{1}{4}$ inches), and the exterior about 202 millimetres (8 inches). After quenching, and having been carefully gauged, the body of the metal was cut away, leaving two exceedingly thin inner and outer annular rings in each case, unsupported, and free to expand or contract.

In the case of the totally quenched tube, the inner zone thus set free was found to have expanded 0·275 per cent. of its diameter, proving that it had been in compression caused by the contraction of the surrounding zones. The exterior zone had expanded 0·158 per cent. of the diameter, and had consequently been cramped in by the interior strains of the metal; whereas, to meet the theoretical requirements of the case, as previously shown, it ought to have been in tension, with a tendency to contract.

Consequently, in a tube completely quenched, the external zones are not only useless, but even prejudicial, as their initial strains are exerted in the same direction as that caused by the explosion of the powder.

In the case of the tube quenched on the interior surface only, the diameter of the internal zone, when freed from the action of the body of the metal, showed an increase of 0·203 per cent. The inner part of the tube had consequently been in compression.

The exterior zone showed a contraction corresponding to 0·074 per cent. of the diameter, and had therefore been in tension.

Taking the *relative* tension of zone 1 as zero, the *relative* tension of zone n is—

$$0\cdot203 \text{ per cent.} + 0\cdot074 \text{ per cent.} = 0\cdot277 \text{ per cent.}$$

Taking the *absolute* values of these tensions:

Zone 1 is, in compression or negative tension, equivalent to $-0\cdot203$ per cent.;

Zone n is, in extension or positive tension, equivalent to $+0\cdot074$ per cent.

The intermediate zone $\frac{n}{2}$ is the only one in equilibrium, or without initial strains.

It is therefore evident that central quenching gives precisely that disposition of strains required to enable every zone of the tube to take its full share of resistance to the strain of explosion.

In order to investigate the manner in which this result is brought about by internal quenching, let us imagine the tube, composed as before of a series of superimposed zones, to be brought from the furnace at a red heat, causing an expansion eight or ten times greater than the maximum elastic limit of the metal under tension. If the central zone is suddenly cooled while the rest retain their heat, it will endeavour to reassume its normal dimensions by shrinking to the full extent to which it has been expanded; but as this contraction is opposed by its cohesion with the whole remaining body of metal, it can only be partial; and the greater the total thickness, the less will be the contraction; so that after it is completely quenched (supposing the other zones to be still at a red heat), its diameter will be greater than it originally possessed; and this increase will consist of two factors:—

1. A permanent extension which may be neglected;
2. A maximum elastic extension.

In this condition therefore, which is only momentary, the first zone is in tension.

As the quenching action continues, the second zone behaves in exactly the same way in relation to the total thickness of the succeeding zones which are still at a red heat, except that they have less thickness to oppose its contraction, which will be greater than that of the first zone. But the moment this contraction takes place, the first zone ceases to be in tension, and is ultimately in compression, with a tendency to expand, were it free to do so.

The contraction of the third zone in turn changes the state of tension of the second zone into one of compression, and causes additional compression of the first zone.

Pursuing the same course of reasoning, it follows that the successive zones take a proportionate grip on each other, and that the whole tube is brought into the desired condition.

It may be similarly shown that, in the case of a tube totally quenched, the zones from 1 to $\frac{n}{2}$ are brought into the condition above described, but that those from $\frac{n}{2}$ to n are in an exactly opposite condition, the initial contraction of zone n on the inner ones becoming first *nil*, and finally negative.

The Paper contains records of experiments on different classes of steel as to contraction and ultimate strength, and is illustrated by diagrams and a plate, showing the apparatus employed for internal quenching.

W. S. H.

The Strength of Rifled Guns. By Col. R. de la Rocque.

(Mémorial de l'Artillerie de la Marine, vol. xiii., 1885, pp. 1–130.)

The Author deals in his first section with the French artillery and its steel guns of the period 1861 to 1870. Guns of above 5 inches bore only are referred to, as that at this time there was no question of the strength of guns of smaller calibre. After describing the trials of guns of various modes of construction, the Author shows that up to 1870, the manufacturers could not deliver steel-blocks from which a safe gun could be constructed.

The second section embraces the period 1872 to 1875, and describes the trial of two guns of 9·44 inches bore, the results of which showed that though great improvements had been made in the manufacture of steel since 1868, still in 1872–3, it was not sufficiently advanced to ensure good tubes of all calibres, or jackets for guns of 9·4 inches bore suitable for service, and in fact that the strength of steel guns of that date was not superior to the cast-iron guns of 1870 then in service.

Section iii. treats of the period 1875–81. Refers to the offer of the Creuzot firm to make a steel gun of 100 tons weight, to be placed in the Paris Exhibition of 1878, and purchased by the French Government at 2*s.* 8½*d.* per lb., and after trial, an order for ten more to be given at 1*s.* 11*d.* per lb. General Frébault's protest against the proposal is quoted at length, with his offer to construct a gun of equal strength, of cast-iron tubed and hooped, at a cost of 5·45*d.* per lb. Though the Creuzot offer was not accepted, it was decided from that date—1875—to make all the large guns of the fleet of steel.

The trials and mode of construction of various guns of from 10·6 inches to 16·5 inches bore are described. The latter was only delivered in 1882, as the Compagnie Forges de St. Chamond were obliged to erect an 80-ton steam-hammer to enable them to make the gun. The Author considers that up to the time of the erection of this hammer, and the trial of the 16·5 inch gun, the introduction of steel had increased the cost of guns threefold, without any increase in ballistic effects. The armament of the fleet was retarded several years, and to avoid risks, unusual precautions, which were superfluous for iron guns, had to be taken.

Section iv. gives the period 1881–5 with few details, but notes the improvements in quality and regularity of the steel supplied by French firms, draws comparisons between the comparatively small bores of 1881, and the larger bores of 1875–9, also the lengths of the guns; concluding with the statement that as regards steel guns the French artillery is in a state of advancement and certainty whilst foreign countries are in an experimental stage.

Passing on to foreign countries and giving a *résumé* of the histories of the Whitworth and Krupp systems (the former only to 1876), he divides the latter into five periods—1851–68, 1869–72, 1872–77, 1879–82, 1882–85. The principal trials and manufactures of these periods are noted, the information being drawn from numerous sources, principally the Prussian Government trials. The introduction of the brown cocoa powders is here noticed, and the consequent reduction in pressure as compared with the old iron guns.

Referring to foreign nations, nearly all of which, whilst recognizing and using steel, have no definite models for their artillery. England is stated to be in a transition state, reference is made to the report of the 1883–4 United States Committee on the Elswick works, and the bulk of this section is taken up with extracts from and remarks on Colonel Maitland's Paper read at the United Service Institution on the 20th of June, 1884. Italy is next taken, and the large guns made by Armstrong are described and criticized.

Russia, at the time of writing, was only in an experimental stage, also the United States.

The Gonzalez Hontoria guns of the Spanish navy are described and illustrated.

The period of guns made with coils of wrought-iron is reviewed. and the bursting of the 38-ton Woolwich, 100-ton Armstrong, and 6-inch "Active" guns are discussed.

In the last chapter, guns on various systems are noticed, including bronze, wrought-iron, Longridge, Hope, and Ordoner guns. These are noted, not because of results of extended trials, "but because they indicate new lines of experiments, abandoned ideas, or more or less beaten tracks which lead to no issue, and should be avoided." In conclusion, the Author enforces the opinion that in artillery matters, the best text-books are of no value; but that practical and continued trials are the only lines on which it is safe to rely.

J. H. R. W.

The Cause of Erosion in the Bore of Guns.

By Captain A. Laufroy, of the Marine Artillery.

(Mémorial de l'Artillerie de la Marine, vol. xiii., 1885, pp. 173–256.)

The Author commences with a proposition already enunciated in several works on ballistics, viz., "That the erosion of the bore of a gun results from the escape of the powder-gases at high tension through narrow orifices, such as the vent or windage of the projectile." The essay is divided into six chapters. (1 and 2) historical, deal with smooth-bore guns, guns with few grooves, and the modern polygroove rifling. After citing numerous experiments with various pieces of the above description, the Author concludes on the polygroove system as follows:—viz., "That erosion always commences in the upper parts of the bore, and is divided into two distinct zones separated by an interval less attacked." The first zone is in the forward part of the cone of the powder-chamber, and often embraces the origin of the grooves. The second, which is always the most important after a prolonged firing, is at a distance from the commencement of the rifling, greater as the powder is more progressive. The lands are always less attacked than the neighbouring grooves, sometimes they are intact. In time, the erosion extends over the circumference of the bore, at the positions of the two zones, but the top is always most injured. Other things being equal, the erosion develops as much more rapidly as the calibre is greater, and more quickly in bronze than in steel. Tool-marks or defects in metal in the upper part only of the powder-chamber are increased rapidly. After prolonged firing, an increase in the diameter of the bore is found chiefly in the shot-chamber, where the greatest erosion occurs. At the same time the ring of the projectile advances further and further up the cone, joining the powder-chamber to the bore.

The length of service of a gun is almost always limited by the loss of velocity and accuracy occasioned by erosion.

The wear of the rings, almost nothing at the commencement of firing, increases with the service of the gun.

For the first round of each series in a gun, the wear of the ring is about double that of the succeeding rounds (Report 826, 1879).

Chapter III. is a theoretical study of the mode of production of erosion in the bores of guns made since 1870.

The Author states that when a gun is fired, it expands under the action of the powder-gas, which exerts a certain tension on the interior of the bore in a circular sense. When the gun is in one piece, this expansion of the bore is proportional to the calibre and interior pressure; according to the formula of General Virgile it is in inverse ratio of the modulus of elasticity, and diminishes when the thickness of the tube increases.

But large guns are composed of several pieces, which, if they have the same modulus of elasticity, require a fresh formula to express the relations of the circular tension and interior pressure in the bore, owing to the shrinkage employed to build up the gun. This formula is given, and the Author, with the object of proving that the erosion commences in the upper part of the bore at the point occupied by the driving ring of the projectile when the maximum pressure occurs, and that all things being equal, it is greater as the calibre increases, proceeds to consider the ring of the projectile in three positions in the bore: (1) In the forward part of the cone at the commencement of the bore. (2) A little to the rear of the last position. (3) At the rear of the cone—the gun being taken as a smooth-bore, with the powder-chamber larger than the bore, and connected with it by a cone.

The second zone of erosion is next dealt with, this being a consequence of the first. The theory that the principal zone is caused by blows from the projectile, is questioned, as in this case the lands should suffer most, whereas it is the grooves that are most injured; it is not denied that blows are given by the projectile, but it is suggested that these will be reduced, as also the escape of gas, by increased forcing of the rotating rings. In considering the wear of the driving-rings, on the hypothesis that the walls of the gun act as a file, for helicoid rifling the Author gives a formula showing the wear to be proportional to the tangent of the final inclination of the grooves and the square of the initial velocity, and considers it advantageous to increase the number of the grooves. To diminish the wear as much as possible, and to preserve the ballistic properties of the gun, the Author suggests a parabolic form of rifling, in which the pressure will be little at the commencement of movement, that is, at the parts most eroded; but in choosing a form of rifling, the wear, and also the maximum circular pressure exerted by the lands on the ring, must be taken into account.

Suggesting an interior design for a gun, the Author adopts for the shot-chamber a cone sufficiently long for the ring to be tightly in contact at the moment of maximum pressure. The angle of this cone has been approximately determined by experiment, the length should be a little more than the distance, which in guns of the same calibre already proved, separates the initial position of the driving-ring and the mass of the erosion. The suggested modifications entail an increase in the diameter of the fillet of the driving-ring, which must be fixed by experiment.

The simplest solution from the manufacturing point of view consists in making a second cone to follow the cone of supports for the ring of the projectile, this cone having a greater inclination, the commencement of the grooves being well in advance of the driving-ring of the projectile when sent home.

J. H. R. W.

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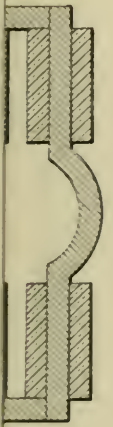

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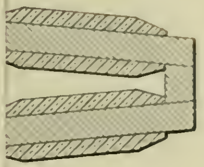

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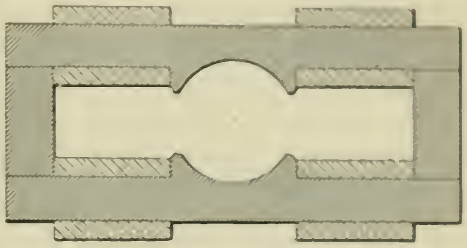

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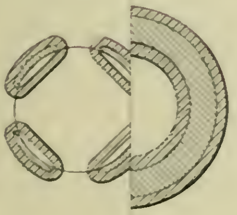

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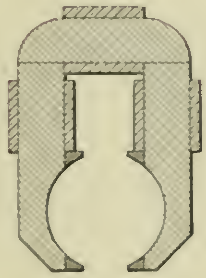

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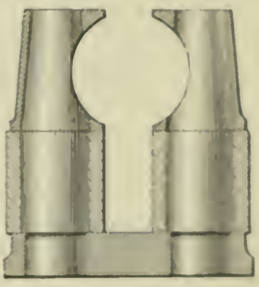

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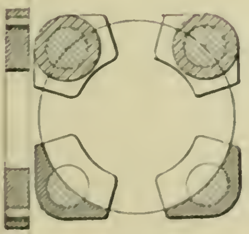

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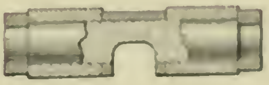

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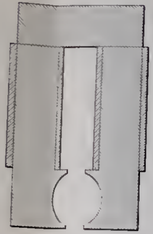
CONTINUOUS-CURRENT DYNAMO-MACHINES.

PLATE. 1.

TYPES OF FIELD—MAGNETS

Fig: 1.

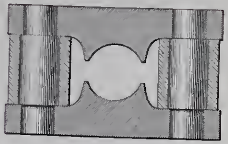
EDISON-HOPKINSON.



DRUM.

Fig: 2.

MANCHESTER.



SHORT CYLINDER.

Fig: 3.

SIEMENS.

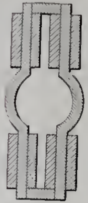


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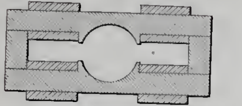
Fig: 4.

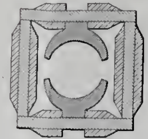
CROMPTON.



CYLINDER.

Fig: 5.

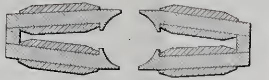
ELWELL-PARKER.



CYLINDER.

Fig: 6.

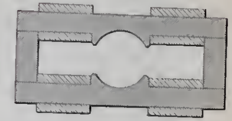
ELWELL-PARKER.



LONG CYLINDER.

Fig: 7.

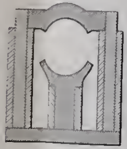
PATERSON AND COOPER.



SHORT CYLINDER.

Fig: 8.

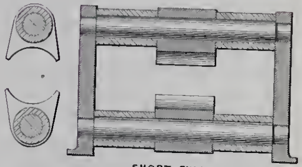
GOOLDEN AND TROTTER.



SHORT CYLINDER.

Fig: 8a.

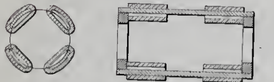
GOOLDEN AND TROTTER.



SHORT CYLINDER.

Fig: 9.

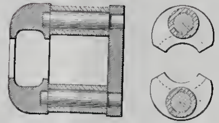
ANDREWS.



SHORT CYLINDER.

Fig: 10.

JONES.



SHORT CYLINDER.

Fig: 11.

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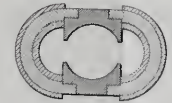

CYLINDER.

Fig: 12.

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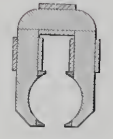

CYLINDER.

Fig: 13.

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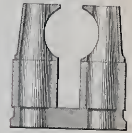

SHORT CYLINDER.

Fig. 14.

WESTON.

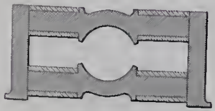

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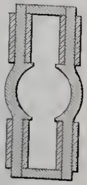
Fig. 15.

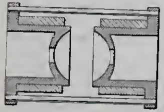
MAXIM.



CYLINDER.

Fig: 16.

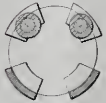
THOMSON-HOUSTON.



SPHERE.

GÜLCHER.

Fig: 17.



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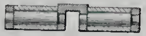

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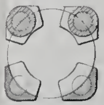

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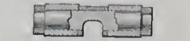

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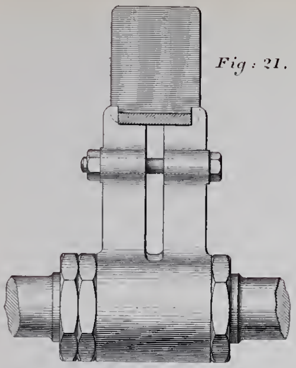
CONTINUOUS-CURRENT DYNAMO-MACHINE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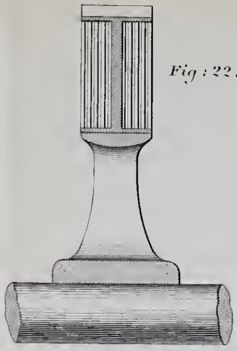
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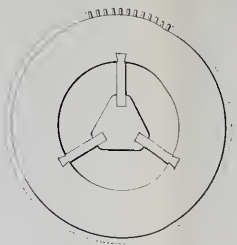
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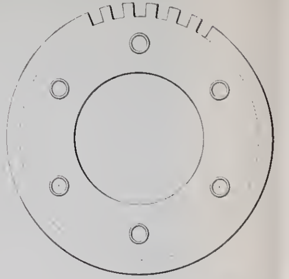
Fig: 24.

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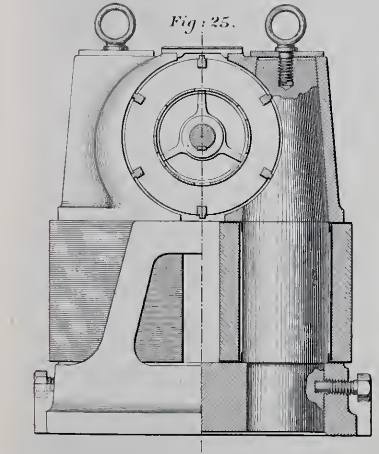
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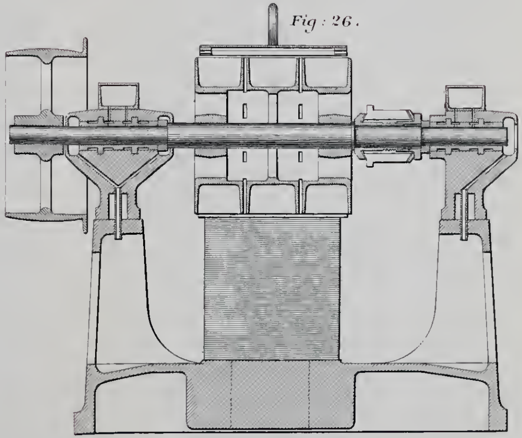
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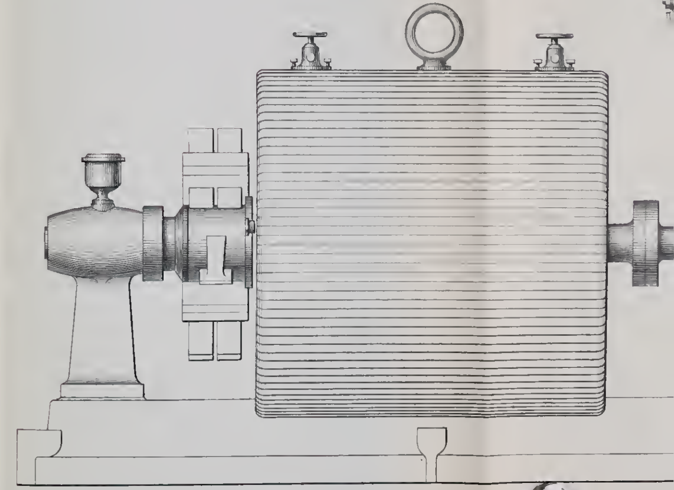

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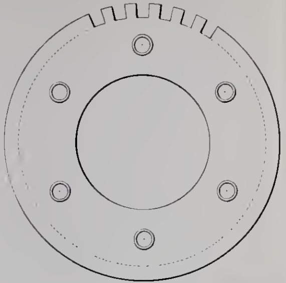
CONTINUOUS-CURRENT DYNAMO-MACHINES.

Fig: 24.



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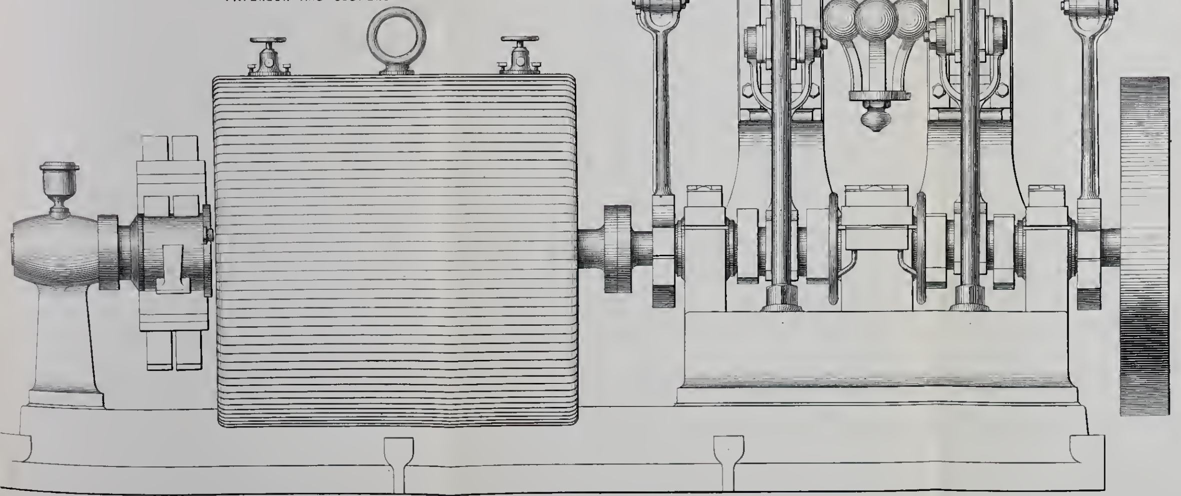

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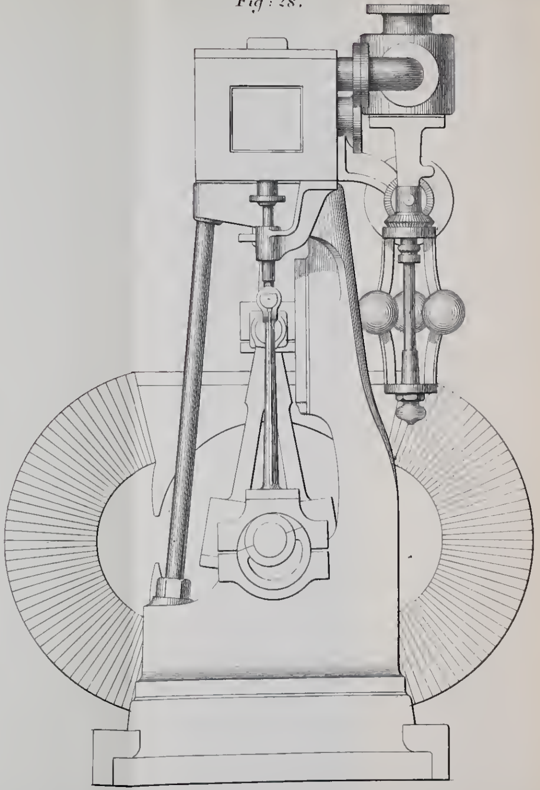

ALLEN ENGINE AND KAPP DYNAMO.

Scale · 1½ Inch = 1 Foot.

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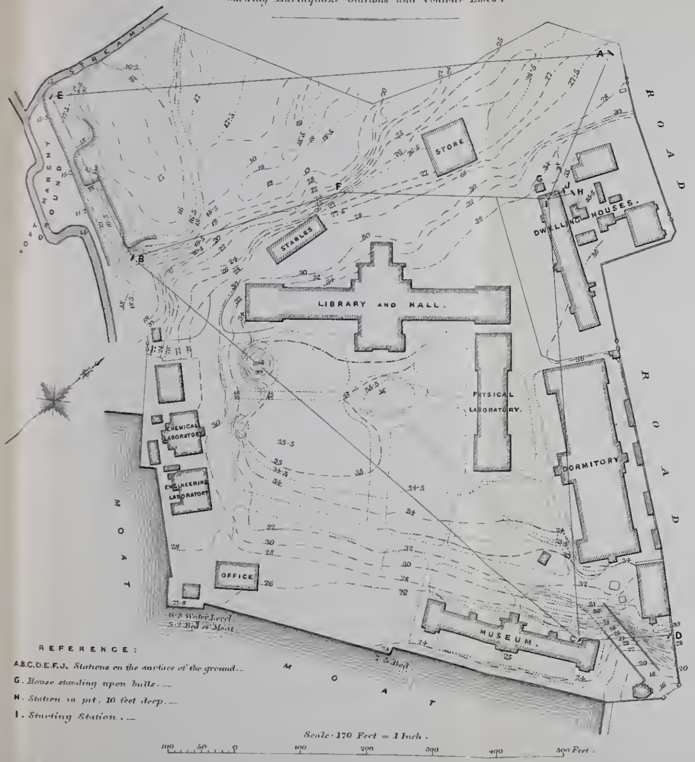
H. BELL & [illegible] LITH 40 KING [illegible] VENT [illegible]EN

GROUNDS OF THE IMPERIAL COLLEGE OF ENGINEERING,
TOKIO, 1885.

Shewing Earthquake Stations and Contour Lines.



REFERENCE:

A.B.C.D.E.F.J. *Stations on the surface of the ground.*—

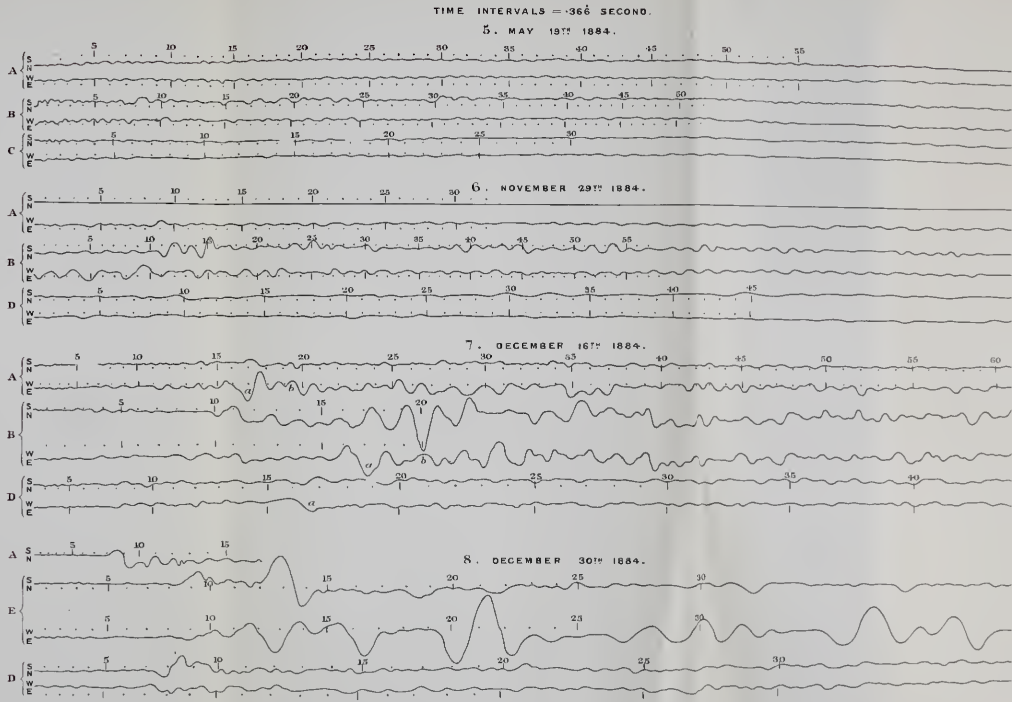
G. *House standing upon balls.*—

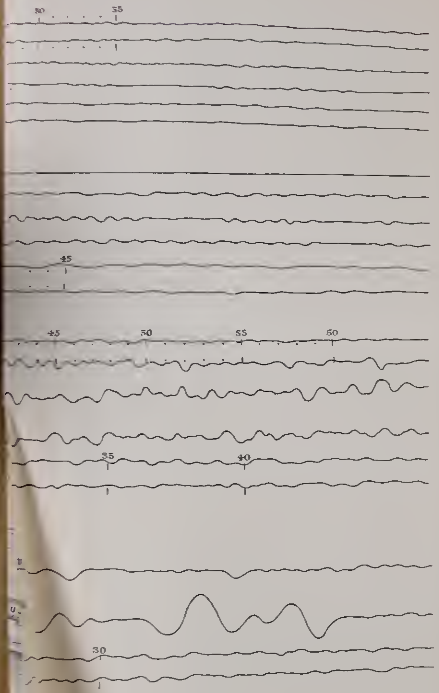
N. *Station in pit, 10 feet deep.*—

I. *Starting Station.*—

J. M^CL MILNE DELT

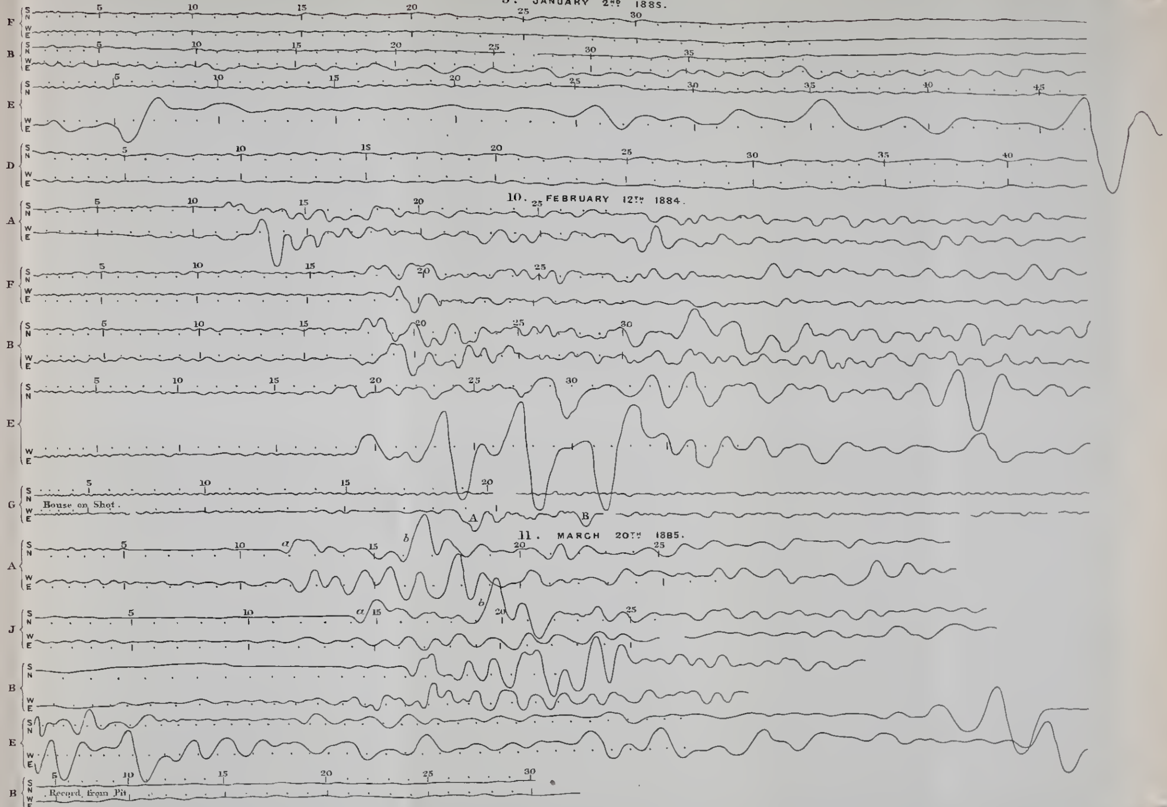
CONSTRUCTION IN EARTHQUAKE COUNTRIES.





TIME INTERVALS = ·366 SECOND.

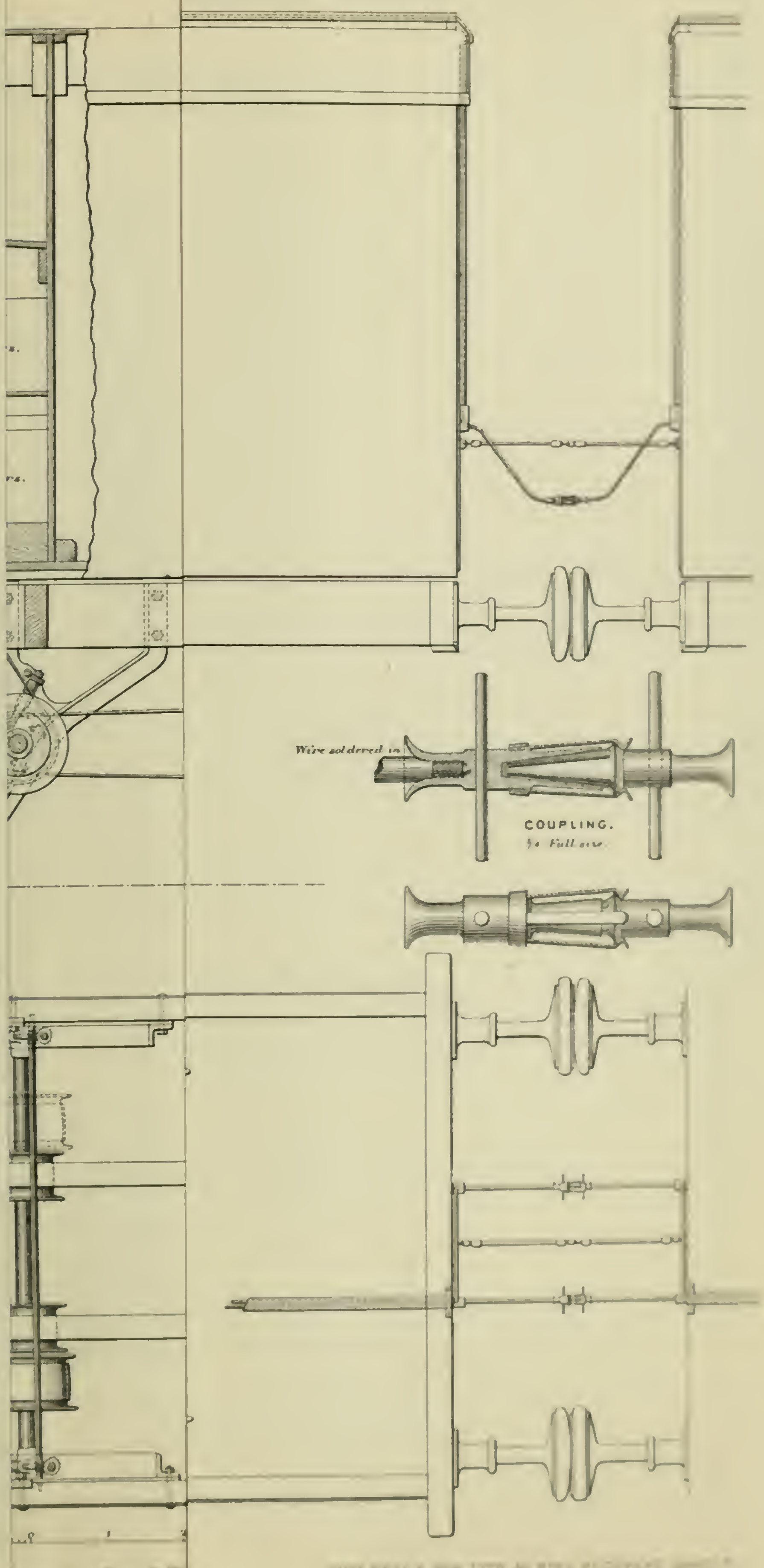
PLATE 3.



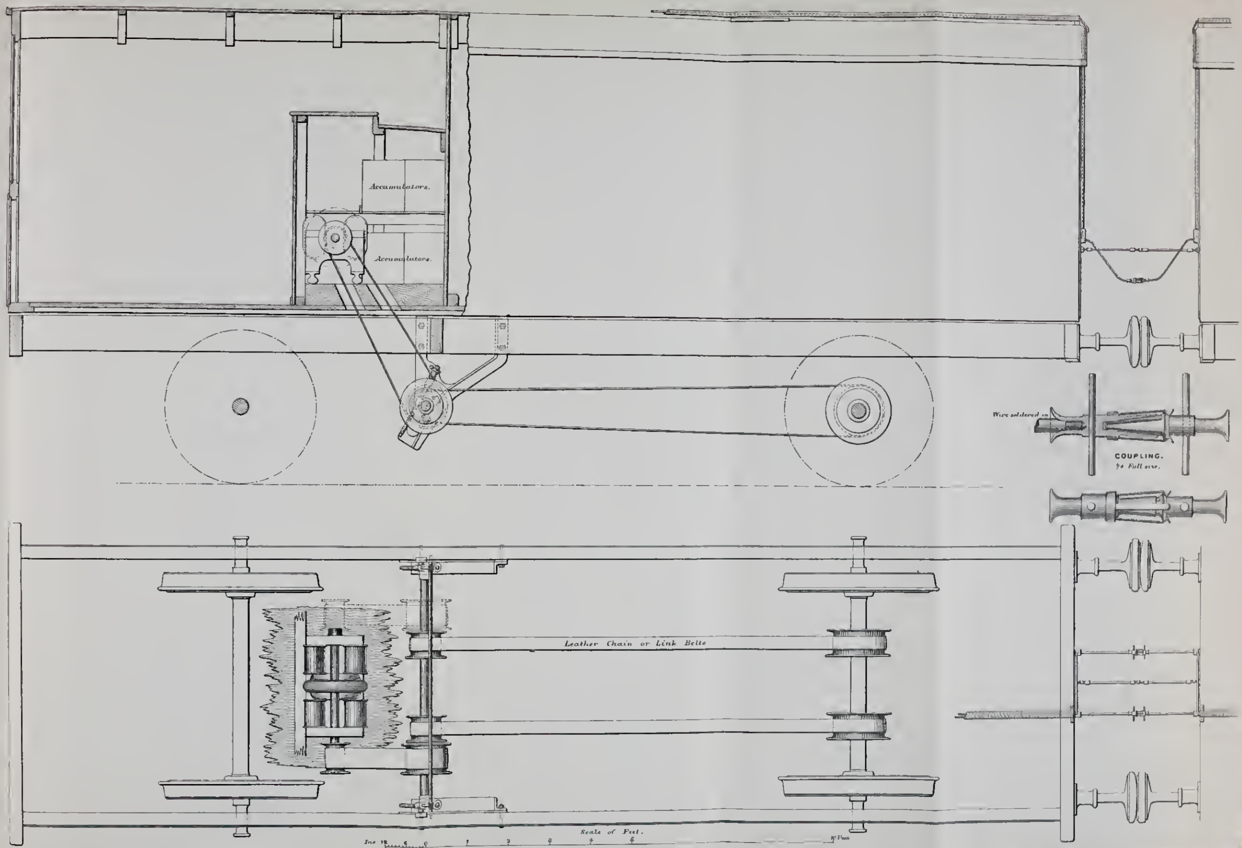
THOS KELL & SON LITH 40 KING ST COVENT GARDEN

LECTRIC

PLATE. 4.



ELECTRIC LIGHTING FOR RAILWAY TRAINS.



W. STROUDLEY, DELT

KARACHI WATER WORKS.

Fig: 1.

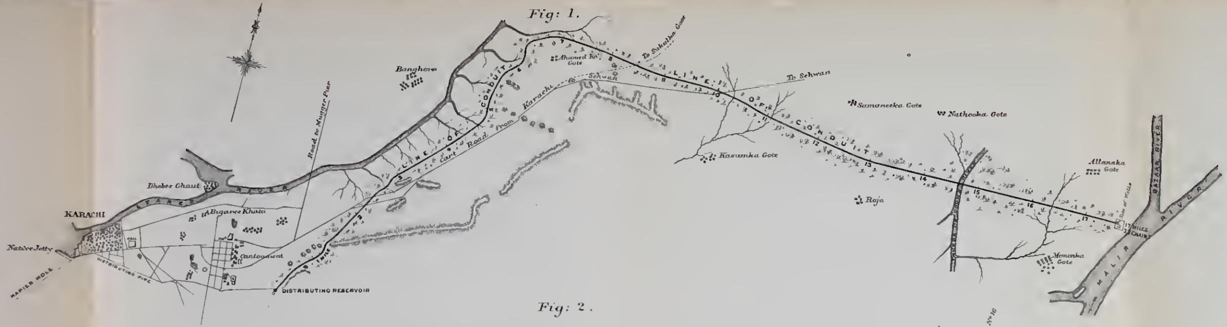
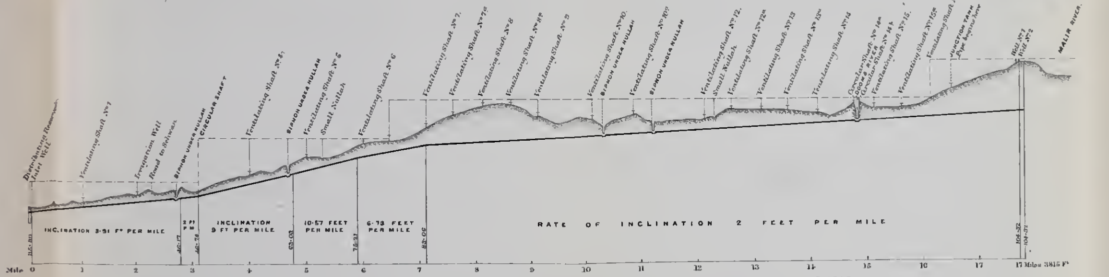


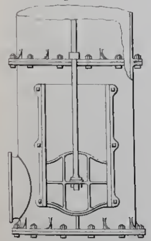
Fig: 2.



SECTION SHOWING THE LINE OF CONDUIT BETWEEN MALIR WELLS AND DISTRIBUTING RESERVOIR.

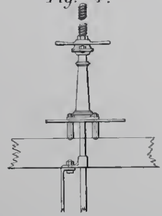
Horizontal Scale
Mile 1 3/4 1/2 1/4 0 1 2 3 4 5 Miles
Vertical Scale
Feet 50 40 30 20 10 0 10 20 30 Feet

Fig: 3.



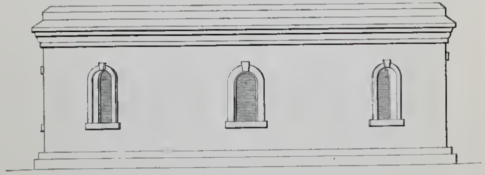
DETAIL OF VALVE SHAFT.

Fig: 4.



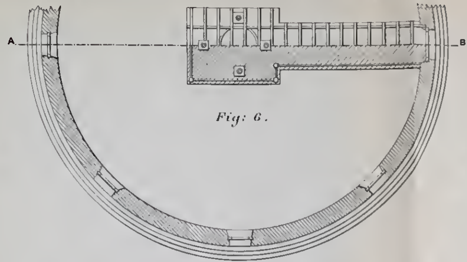
DETAIL OF C.I. COLUMN.

Fig: 5.



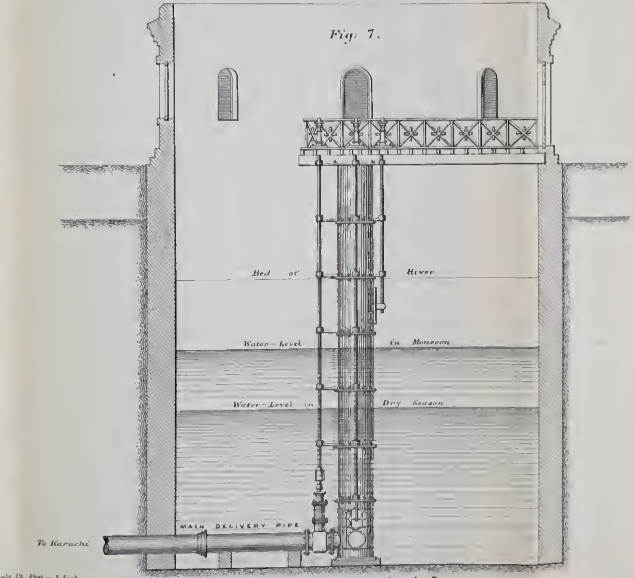
ELEVATION.

Fig: 6.



PLAN.

Fig: 7.



CROSS SECTION ON A. B.

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KARACHI WATER WORKS.

PLATE. 5

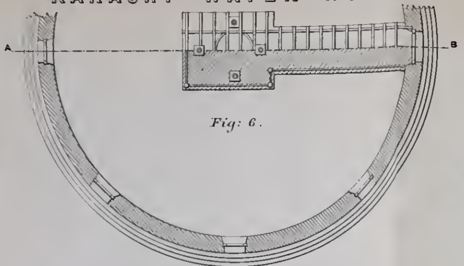


Fig: 6.

PLAN.

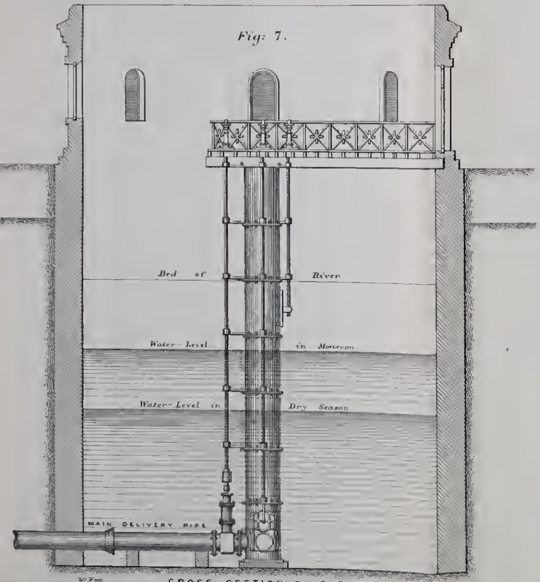
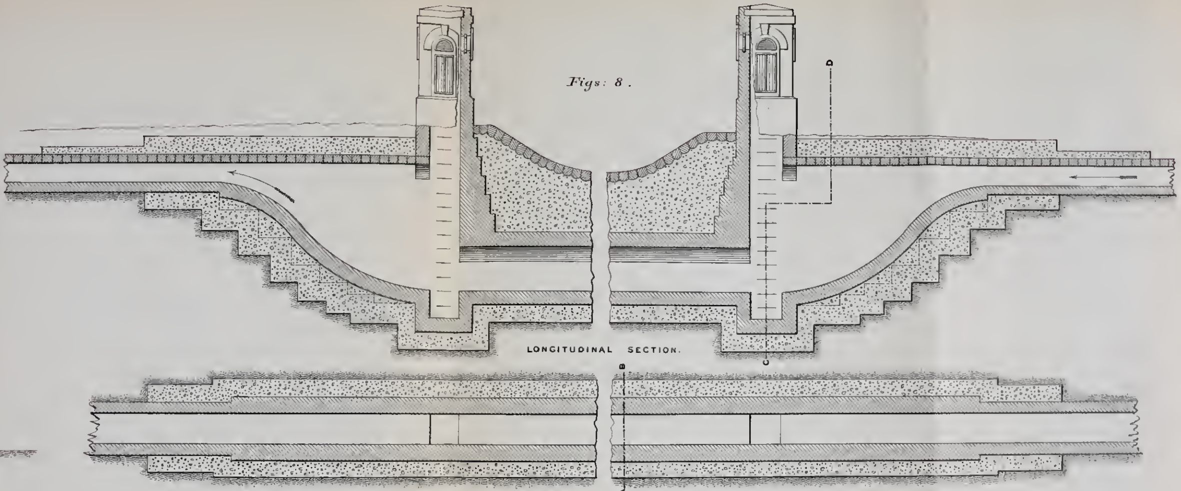


Fig: 7.

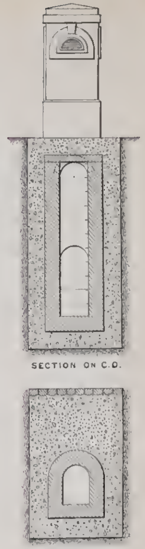
CROSS SECTION ON A.B.

Figs: 8.



LONGITUDINAL SECTION.

GROUND PLAN.



SECTION ON C.D.

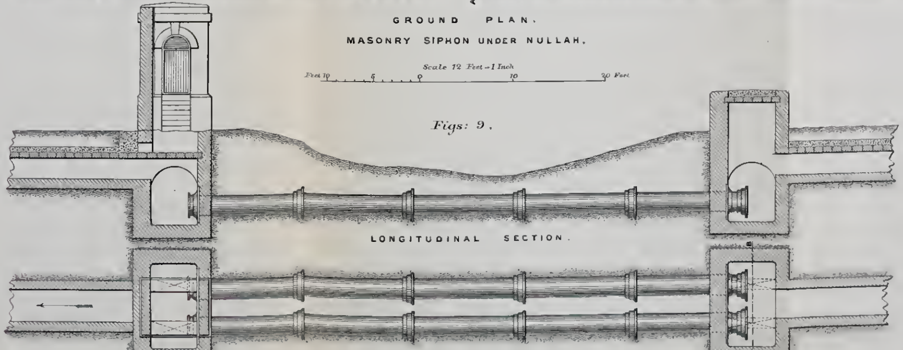
SECTION ON A.B.

MASONRY SIPHON UNDER NULLAH.

Scale 12 Feet = 1 Inch

Feet 10 5 0 10 20 Feet

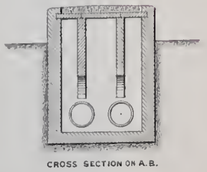
Figs: 9.



LONGITUDINAL SECTION.

PLAN.

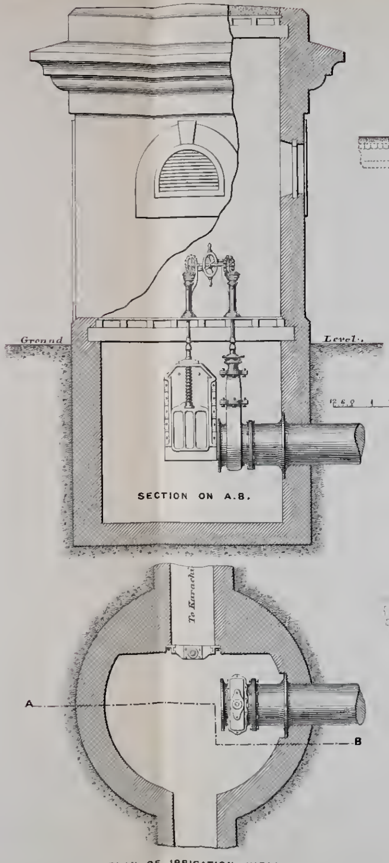
PIPE SIPHON UNDER NULLAH.



CROSS SECTION ON A.B.

KARACHI WATER WORKS.

Figs: 10.



Figs: 11.

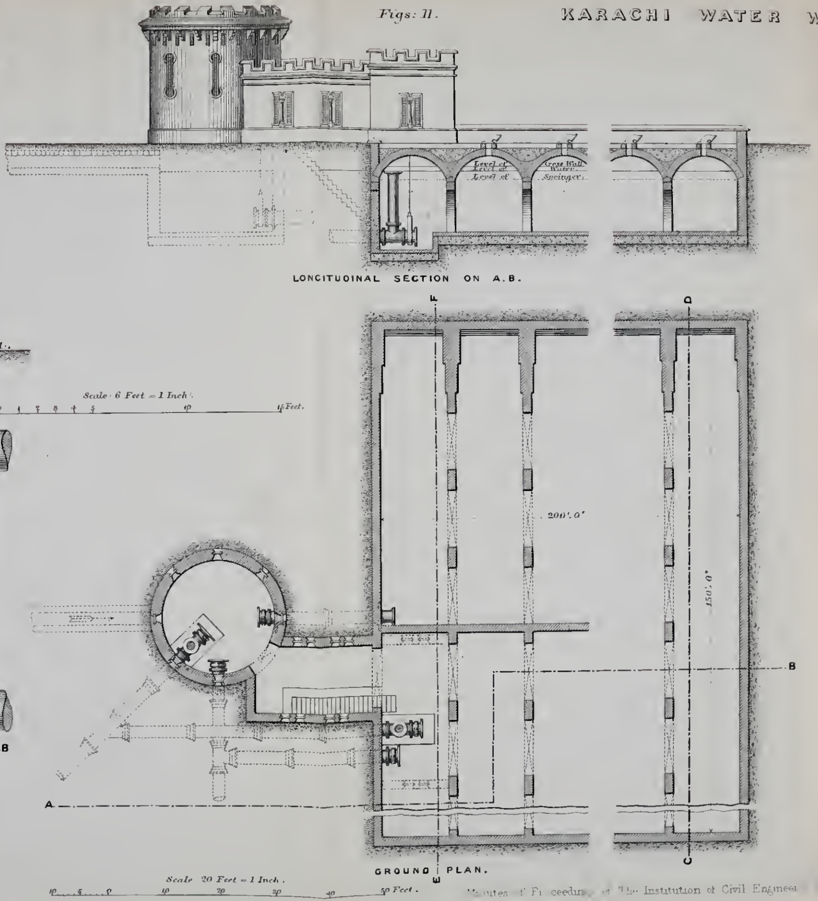
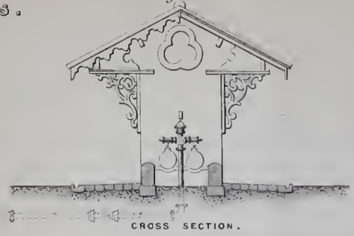
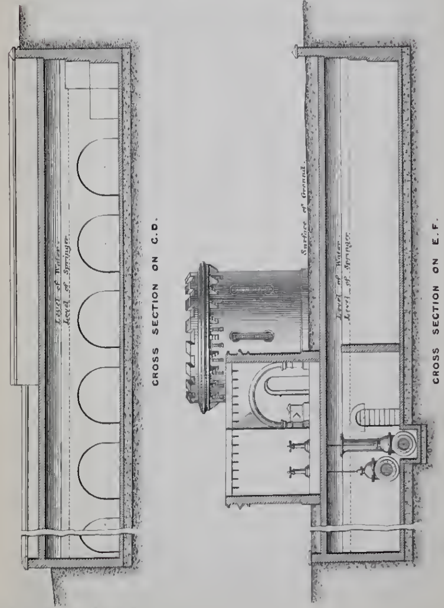


Fig: 13.



Figs: 12.



J. STRACHAN, DELT.

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KARACHI WATER WORKS.

PLATE 6.

Figs: 11.

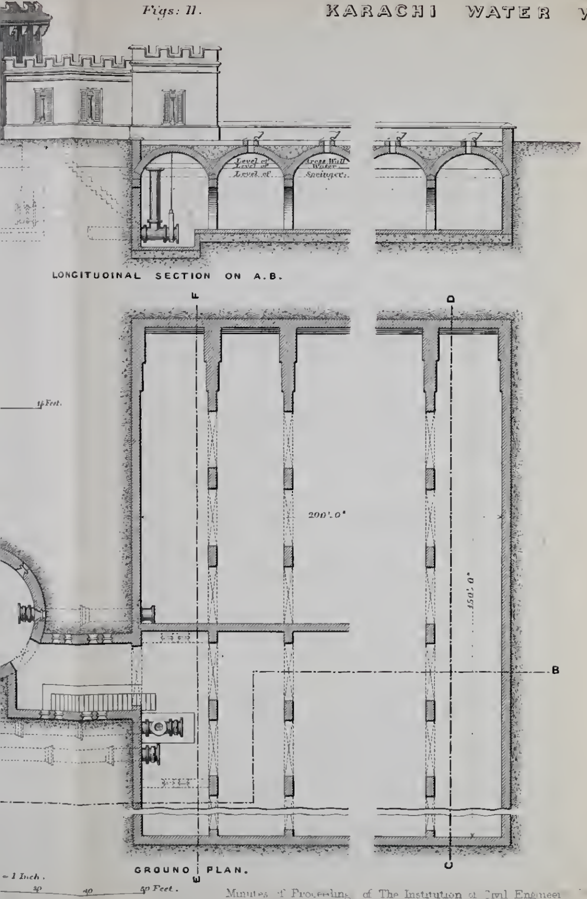
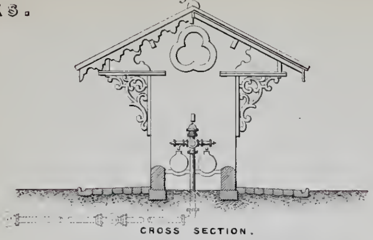
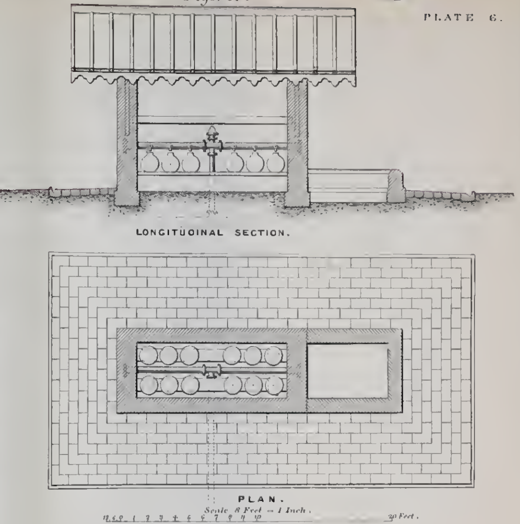


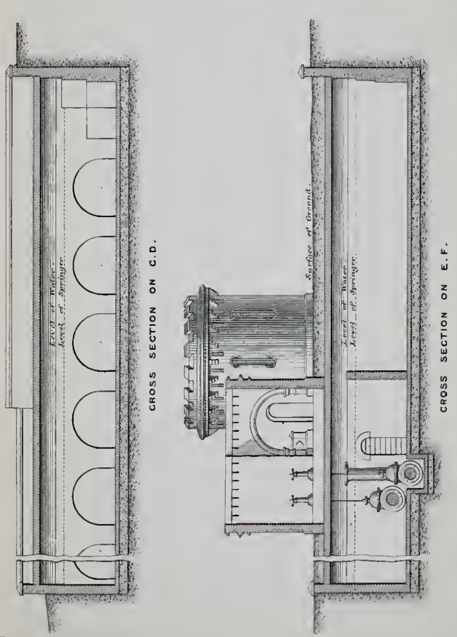
Fig: 13.



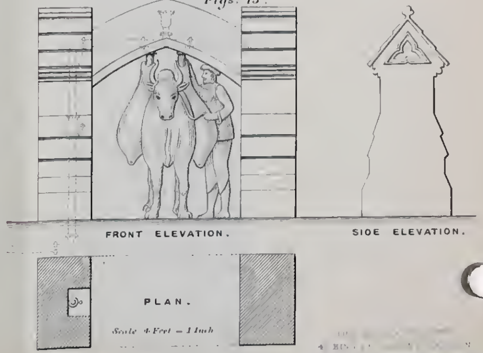
Figs: 14.



Figs: 12.



Figs: 15.



ITALIAN ROLLING STOCK.

ENGINES.

Fig: 1.

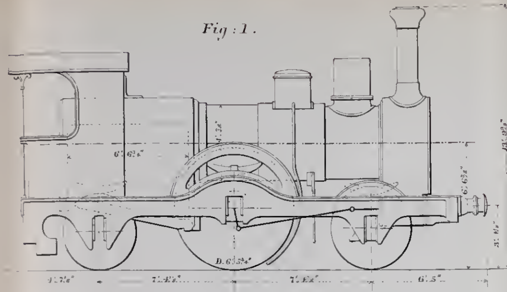


Fig: 2.

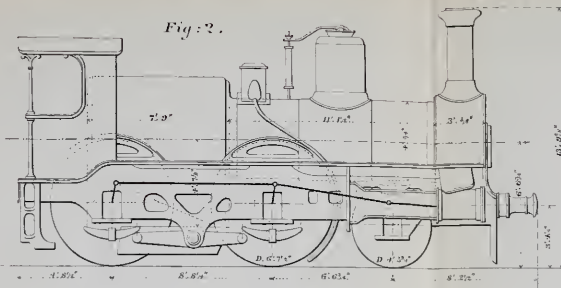


Fig 4.

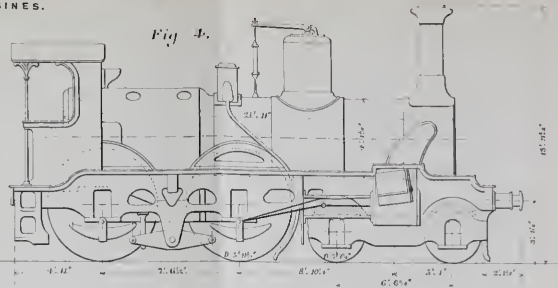


Fig: 3.

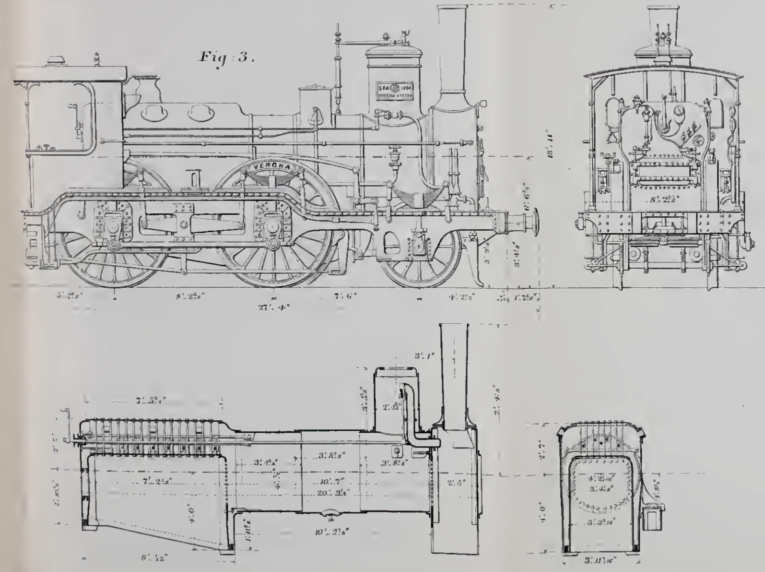
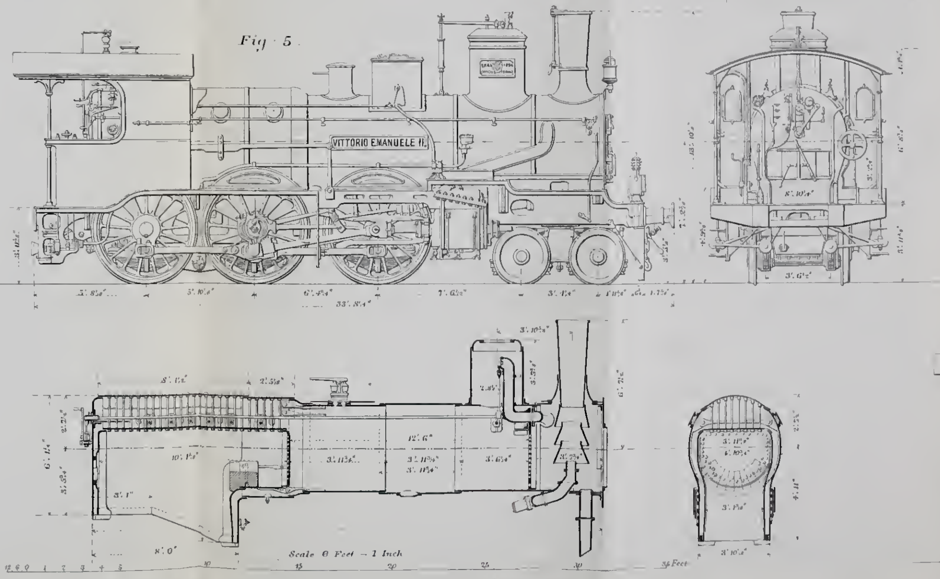


Fig 5.



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Minutes of Proceedings of The Institution of Civil Engineers. Vol LXXXIII Session 1885-86 Part I

ITALIAN ROLLING STOCK.

ENGINES.

Fig: 2.

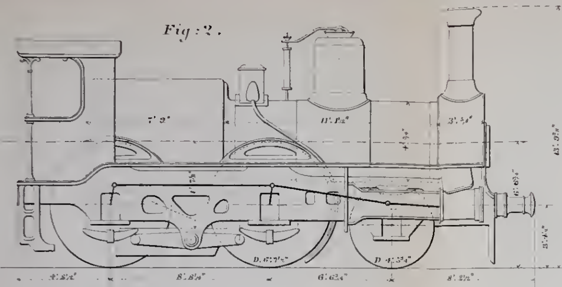


Fig 4.

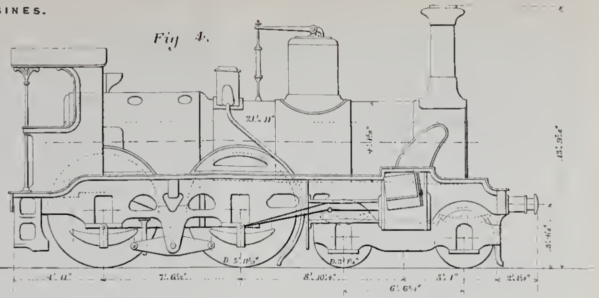
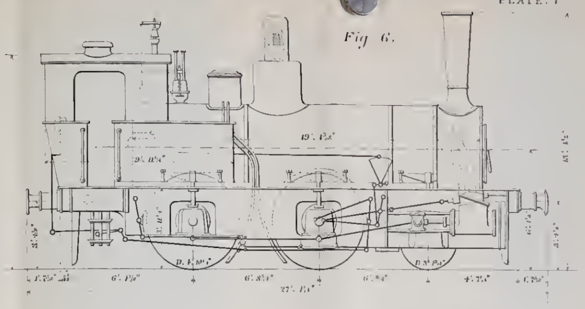


Fig 6.



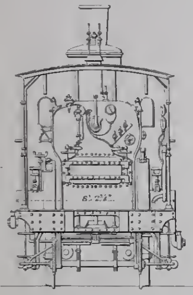
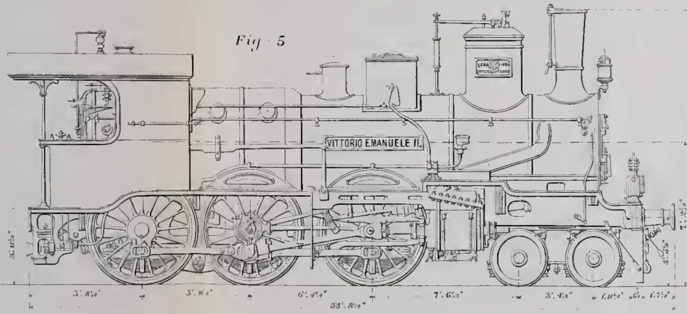


Fig 5



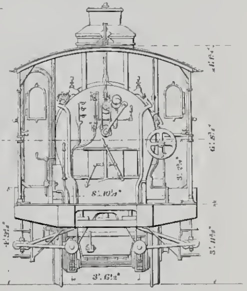
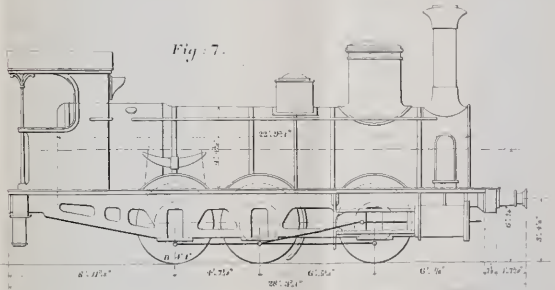
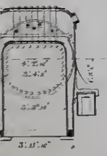
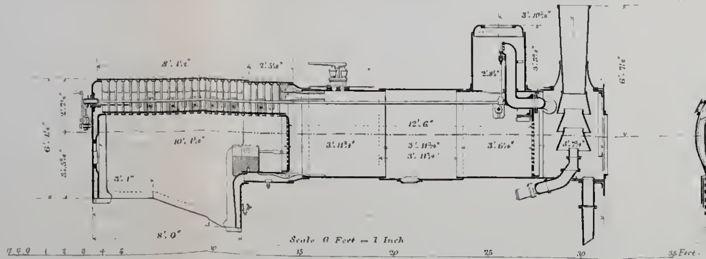


Fig: 7.







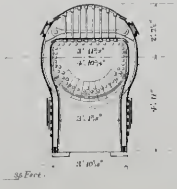
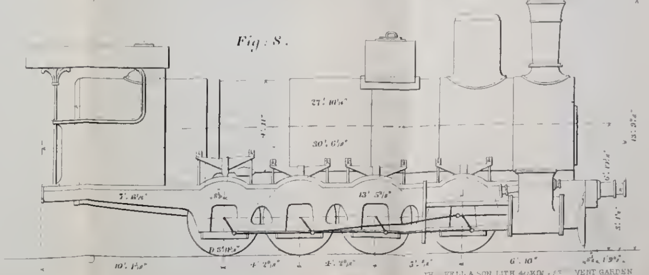


Fig: 8.



THOS KELL & SON, LITH 40 KING ST COVENT GARDEN

ITALIAN ROLLING STOCK.

CARRIAGES.

Fig: 9.

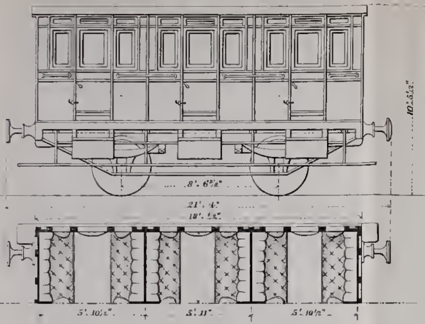
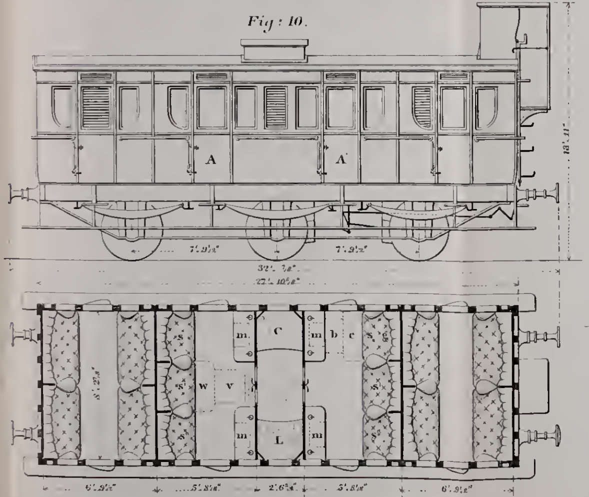


Fig: 10.



Scale : 6 Feet = 1 Inch.

6 4 2 0 1 2 3 4 5 10 15 20 25 Feet.

Fig: 11.

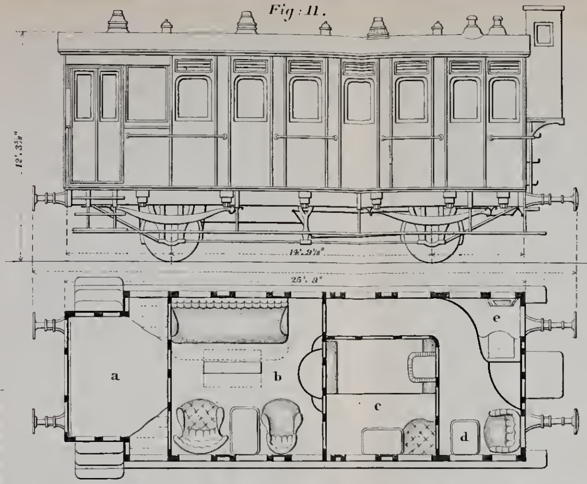


Fig: 12.

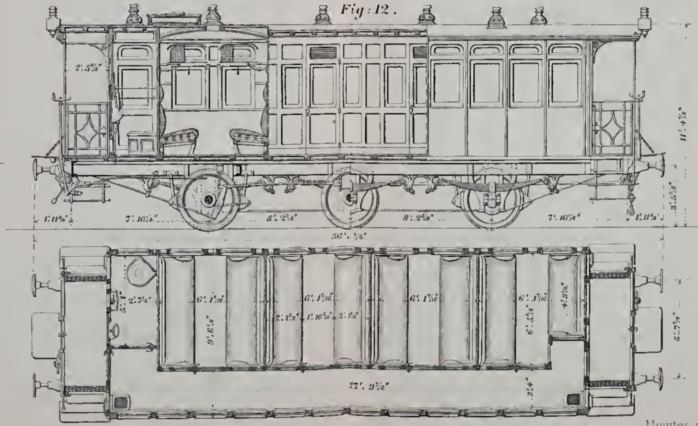


Fig: 13.

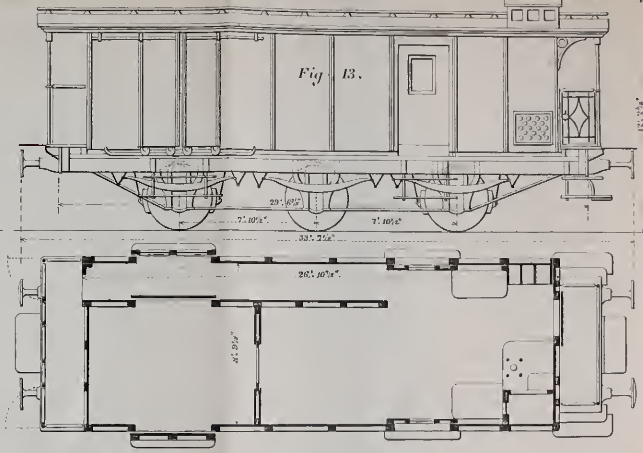
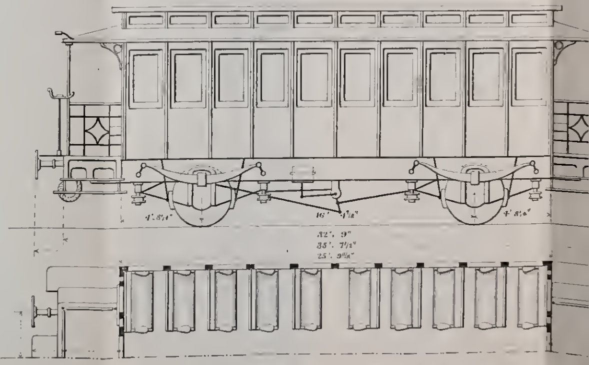


Fig: 14.



PADDA DEL^T

610

AN ROLLING STOCK.

CARRIAGES.

PLATE 8.

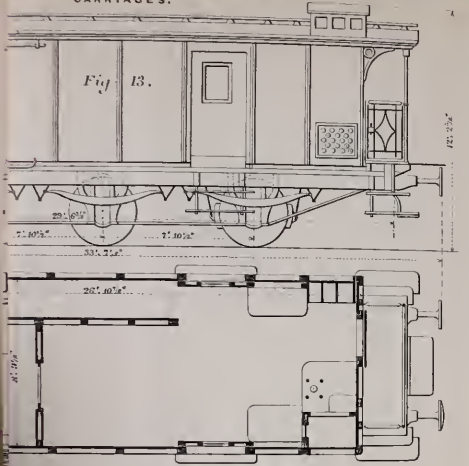


Fig: 14.

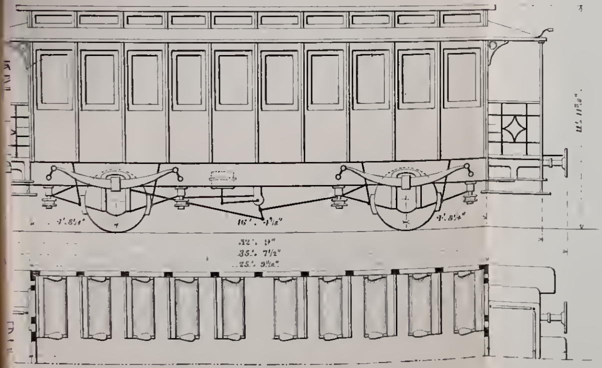


Fig: 15.

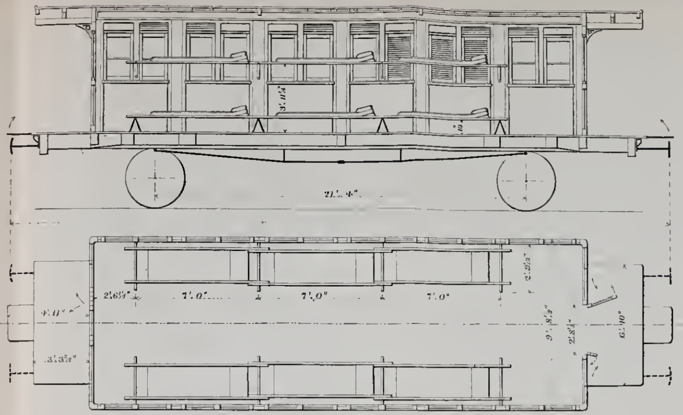


Fig: 16.

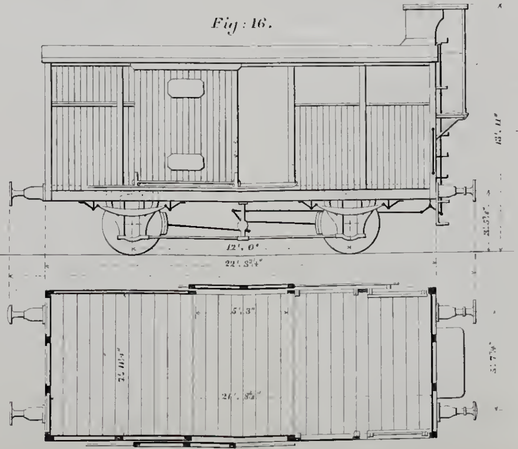


Fig: 17.

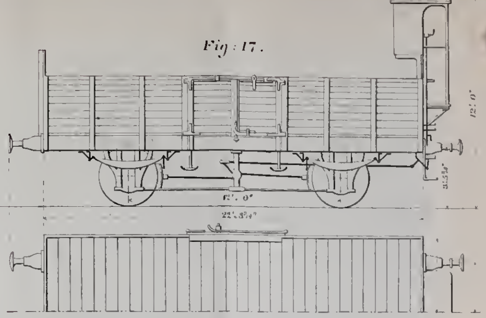


Fig: 18.

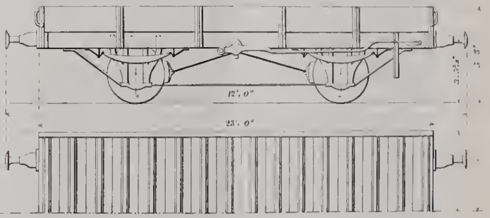


Fig: 19.

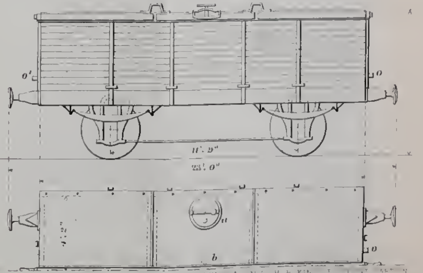


Fig: 20. FTELEY & STEARNS.

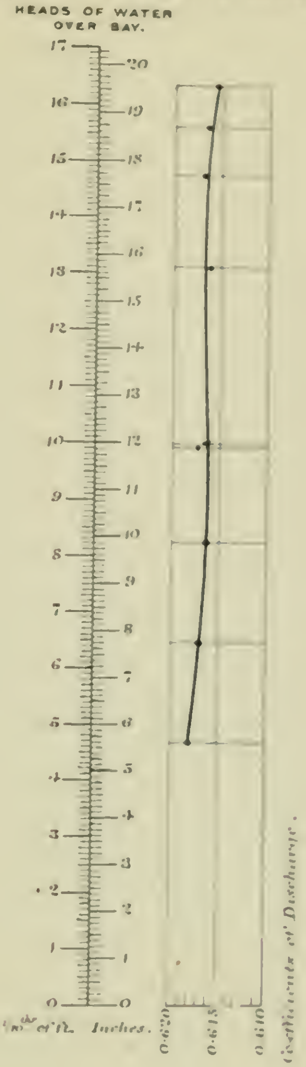


Fig : 19.

FTELEY & STEARNS.

Formula used for determining Coefficient $Q = C L H^{3/2}$; this is divided by 5·3472 for comparison with the other experiments.

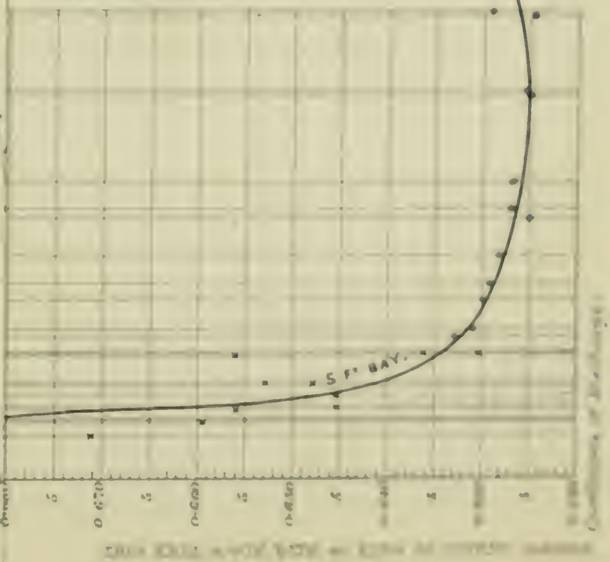
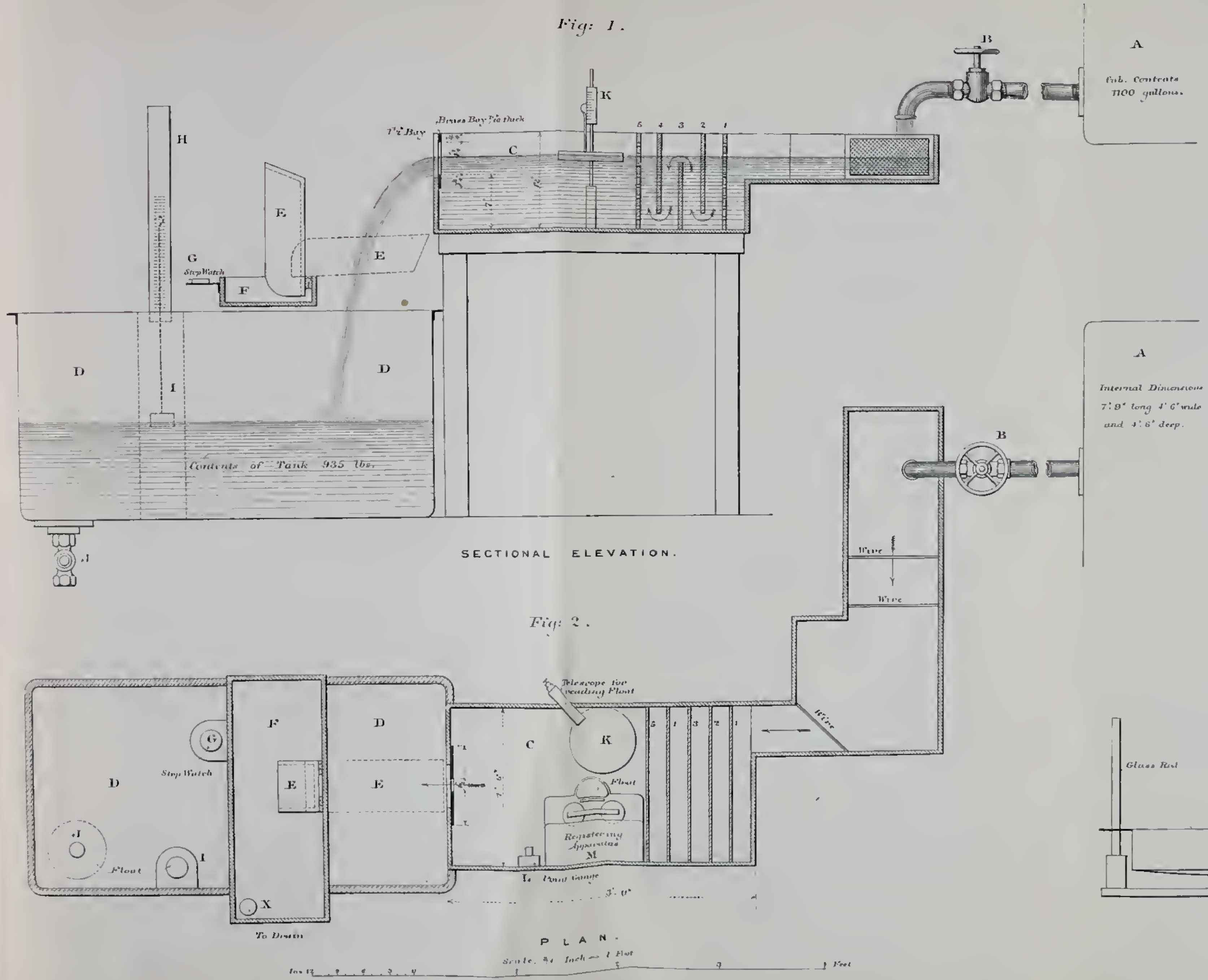


PLATE. 9.

MEASUREMENT OF WATER OVER WEIRS.

TUMBLING-BAY APPARATUS FOR MEASURING WATER.

Fig: 1.



H
G
Stop Watch
F
E
E
D
D
I
J
Contents of Tank 935 lbs.
1½" Bay
Brass Bay ⅛" thick
C
K
5
4
3
2
1
B
A
Cub. Contents 1100 gallons.
A
Internal Dimensions 7' 9" long 4' 6" wide and 4' 6" deep.
B
Wire
Wire
Wire
Telescope for reading Float
G
Stop Watch
F
D
E
E
C
K
Float
Registering Apparatus
M
L
Point Gauge
J
Float
I
X
To Drain
3' 0"
Ins 12 9 6 3 0 1 2 3 Feet

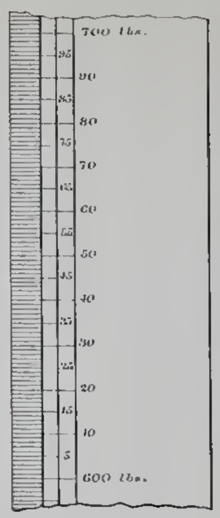
SECTIONAL ELEVATION.

Fig: 2.

PLAN.

Scale, ¾ Inch = 1 Foot

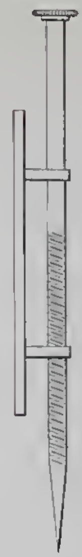
Fig: 5.



SCALE H.

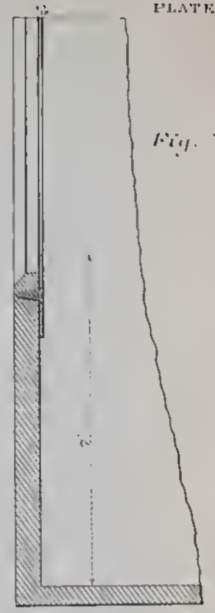
Full Size

Fig: 6.



POINT GUAGE.

Fig. 7.

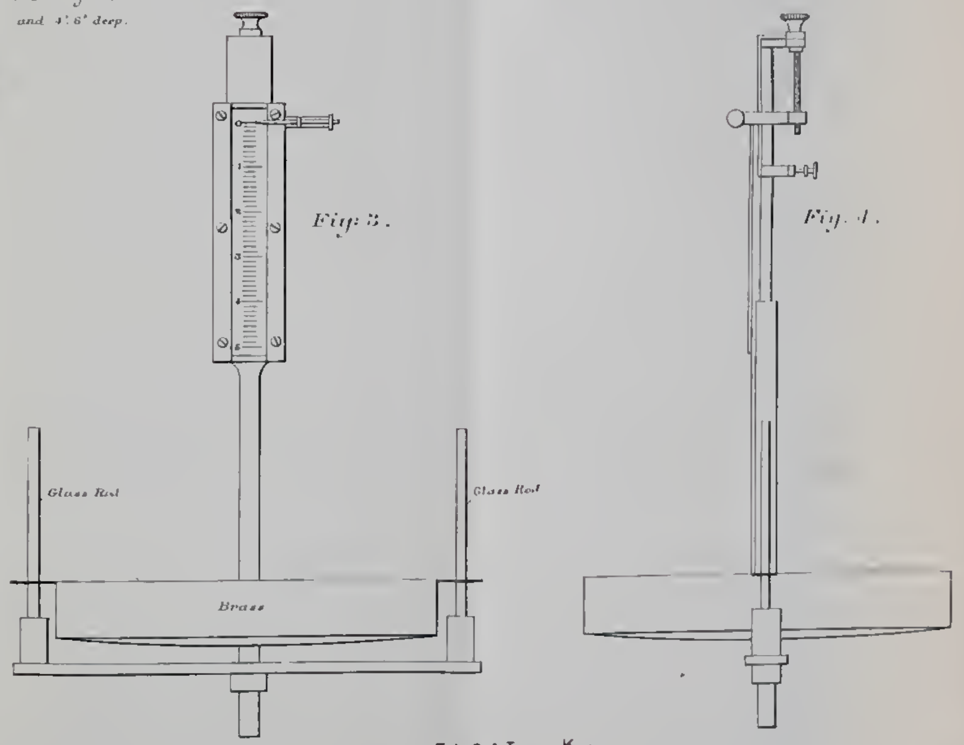


SECTION OF 1½ BAY BRASS.

Scale, ¼ Full Size.

Fig: 3.

Fig. 4.



FLOAT K.

Scale ¼ Full Size

B. DONKIN & F. SALTER, DELᵀ

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THOˢ KELL & SON, LITH 40 KING Sᵀ COVENT GARDEN

MEASUREMENT OF WATER OVER WEIRS.

Fig: 8. SMEATON.

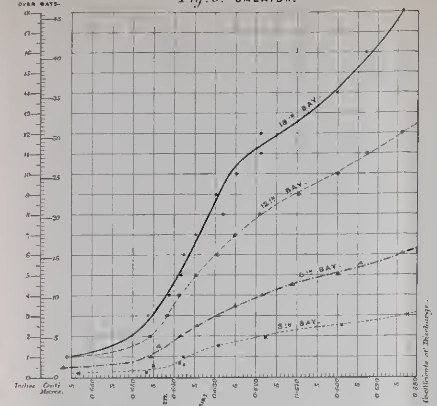


Fig: 9. SMEATON & BRINDLEY.

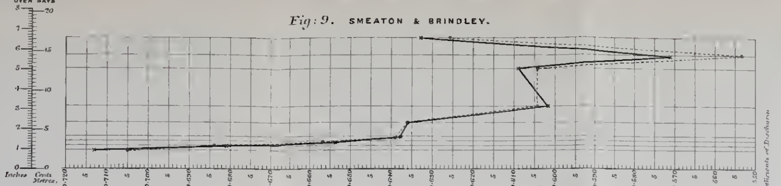
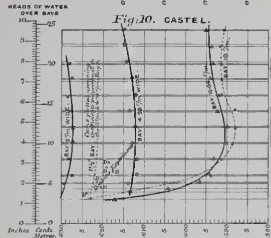


Fig: 10. CASTEL.

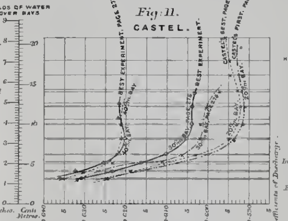


Experiments made on same Bay & Canal {Full Curve, Table 3, page 276 best Experiment. / Dotted Curve, Table 2, page 275 first Experiment.

Formula used by Castel, page 250.
{$Q = \frac{2}{3}\sqrt{2g}\,n\,L\,H\sqrt{H}$ or
$Q = 2{\cdot}953\,n\,L\,H\sqrt{H}$.

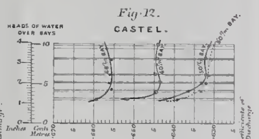
All in Metres
{Q = Quantity per second
L = Length of Bay
H = Head over Bay.
g = 9·809 metres
n = Coefficient.

Fig: 11. CASTEL.



Formula.
{$Q = \sqrt{2gH} \times lH \times m$.
Q = discharge in cub ft. per second.
$\sqrt{2g} = 8{\cdot}03$.
H = Head over Bay in feet.
l = length of Bay
m = Coefficient of correction.

Fig: 12. CASTEL.



Experiment made on same Bay & Canal {Full Curve, Table 3, page 277 best / Dotted Curve Table 2, page 276

Fig: 13. LESBROS.

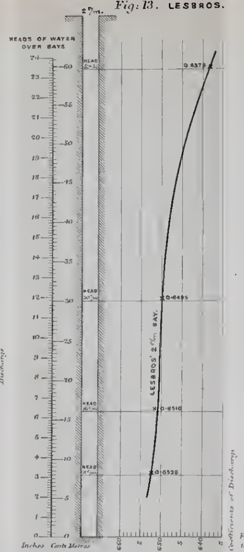
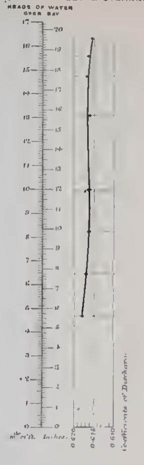


Fig: 20. FTELEY & STEARNS.



Formula used.

CASTEL	LESBROS.
$Q = \frac{2}{3}\sqrt{2g}\,LH\sqrt{H}$ g = 9·809 metres	$Q = LH\sqrt{2g\,H}$ g = 9·8088 m

Fig: 14. CASTEL & LESBROS.

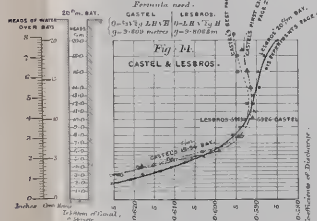


Fig: 15. BALLARD.

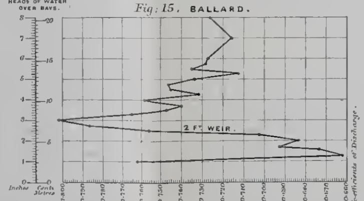


Fig. 16. BLACKWELL.

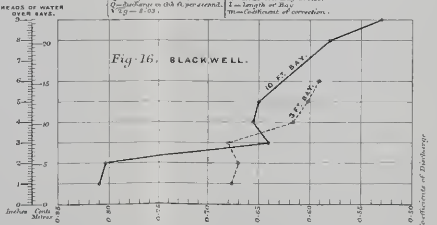
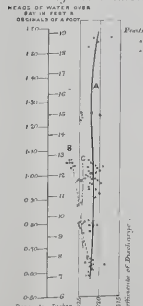


Fig: 17. FRANCIS.



Positions thus • Curve —— A
" " ○ " B
" " ○ " - - - C

Same Canal {A. Canal little wider than Weir / B. " " " " " bottom raised to 2 feet from underside of Bay / C. Canal & Weir the same width

Fig: 18. DONKIN & SALTER.

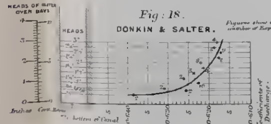
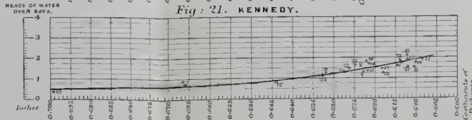


Fig: 21. KENNEDY.



Results of Experiments made at the Engineering Laboratory University College, London, during October & November 1884. The No. opposite the small circles indicate the number of the Experiment readings of the head were taken every 15 seconds. Same Formula used as by Donkin & Salter.

NUMBER OF EXPERIMENT	OBSERVED HEAD INCHES	DURATION MINUTES	COEFFICIENT
1	1·327	8	·6277
2	1·289	8	·6228
3	rejected	0	0
4-7	0·606	12	·6624
8-9	1·304	6	·6120
10	0·814	5	·5995
11	1·605	5	·6284
12	0·832	7	·6441
13	2·073	5	·6118
14	1·682	5	·6212
15	[illegible]	8	·6220
16	1·770	7	·6139

NUMBER OF EXPERIMENT	OBSERVED HEAD INCHES	DURATION MINUTES	COEFFICIENT
17	1·483	8	·6183
18	1·770	6	·6088
19	1·285	8	·6181
20	1·533	8	·6121
21	1·577	8	·6101
22	1·944	8	·6048
23	1·846	6	·6139
24	1·141	6	·6321
25	1·652	7	·6287
26	1·760	6	·6214
27	1·466	8	·6265
28	1·255	10	·6320

Fig: 22. BOILEAU.

Formula used
{Q = Coefficient $LH\sqrt{2gh}$
L = Length of Bay.
H = Head over Bay.

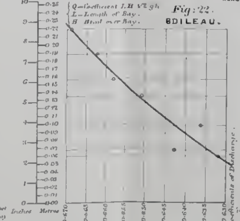
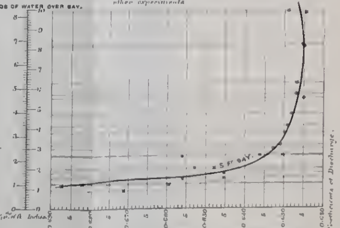


Fig: 19. FTELEY & STEARNS.

Formula used for determining Coefficient $Q = C\,L\,H^{3/2}$ then is divided by 5·3472 for comparison with the other experiments



CONCRETE-BUILDING AT SIMLA, INDIA.

Fig 1

PLAN OF ARMY HEAD-QUARTERS

Scale 36 Feet = 1 Inch

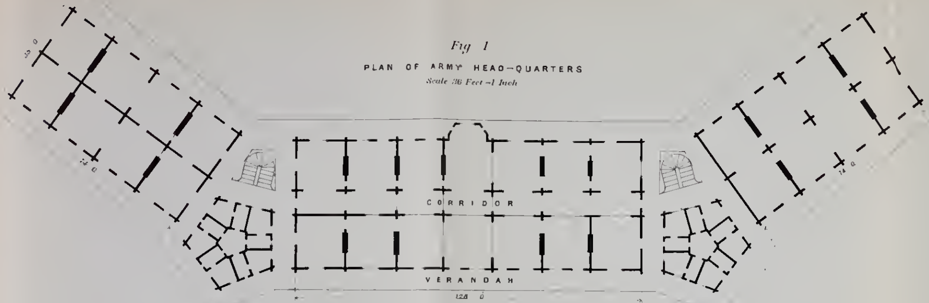
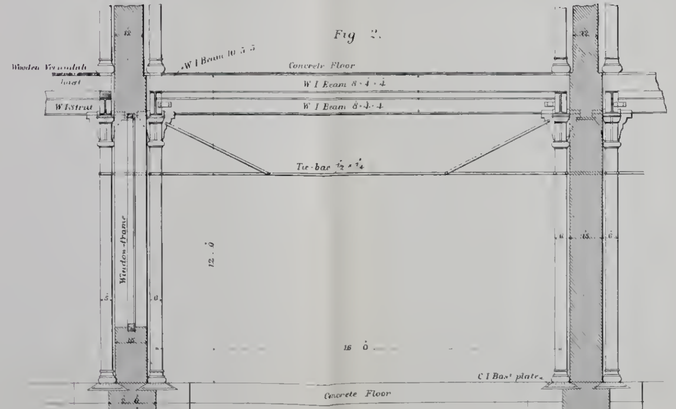


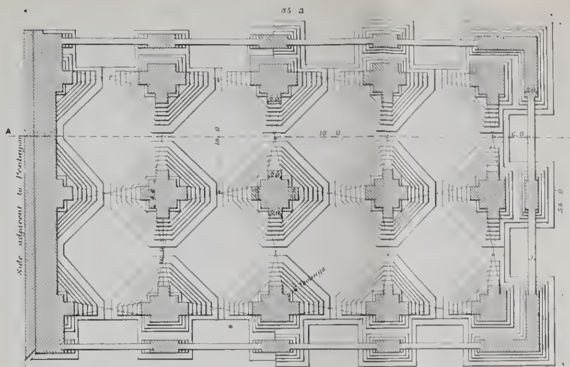
Fig 2.



CROSS-SECTION OF BASEMENT ROOM

Scale 1/4 Inch = 1 Foot

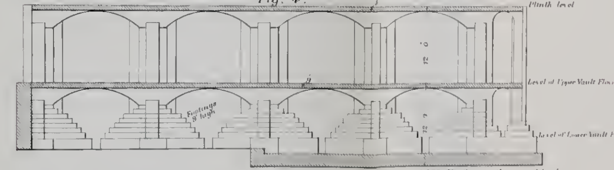
Fig 3.



FOUNDATIONS OF EAST WING ARMY HEAD-QUARTERS

Scale 1/16 Inch = 1 Foot

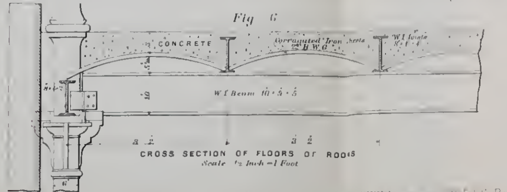
Fig: 4.



LONGITUDINAL SECTION ON A B

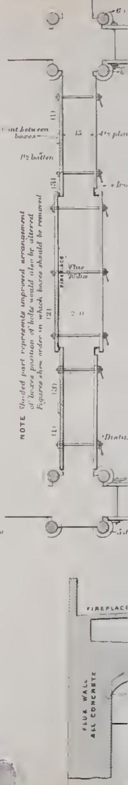
NOTE *The fig gives the general level of foundation of piers concrete in bad spots extended below this*

Fig 6



CROSS SECTION OF FLOORS OF ROOFS

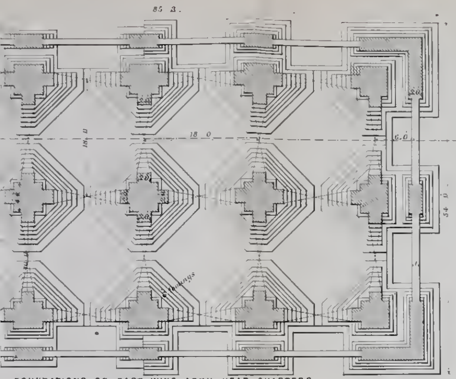
Scale 1/2 Inch = 1 Foot



NOTE *Shaded part represents improved arrangement of boxes position of bolts would also be altered Figures show order in which boxes should be removed*

RETE—BUILDING AT SIMLA, INDIA.

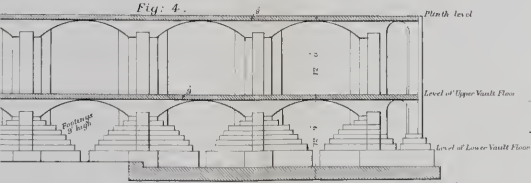
Fig 3.



FOUNDATIONS OF EAST WING ARMY HEAD—QUARTERS

Scale 1/16 Inch = 1 Foot

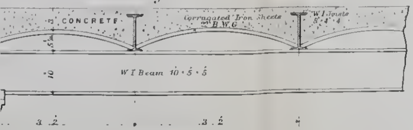
Fig: 4.



LONGITUDINAL SECTION ON A B

NOTE The fig gives the general level of foundation of piers, concrete in bad spots extended below this

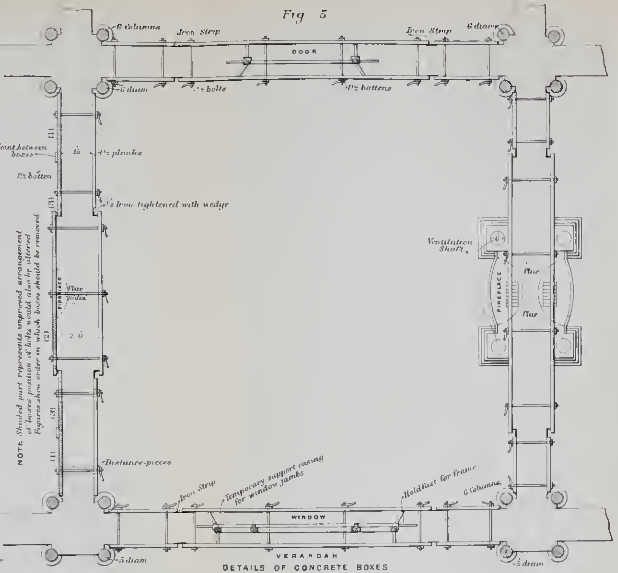
Fig: 6.



CROSS-SECTION OF FLOORS OF ROOMS

Scale 1/2 Inch = 1 Foot

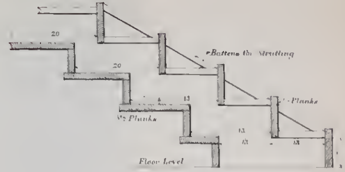
Fig 5



DETAILS OF CONCRETE BOXES
ARRANGEMENT FOR THE DIFFERENT WALLS

Scale 1/4 Inch = 1 Foot

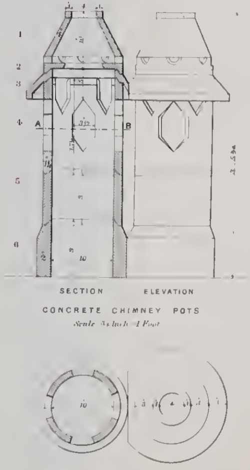
Fig 7



BOXING FOR STEPS MAINSTAIRS

Scale 1/2 Inch = 1 Foot

Figs 8



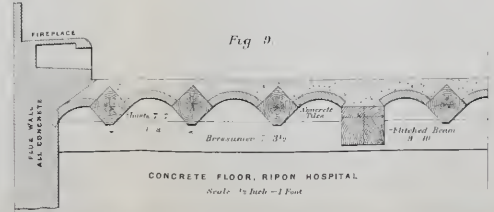
SECTION ELEVATION

CONCRETE CHIMNEY POTS

Scale 3/4 Inch = 1 Foot

PLAN AT A B PLAN AT TOP

Fig 9.



CONCRETE FLOOR, RIPON HOSPITAL

Scale 1/2 Inch = 1 Foot

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